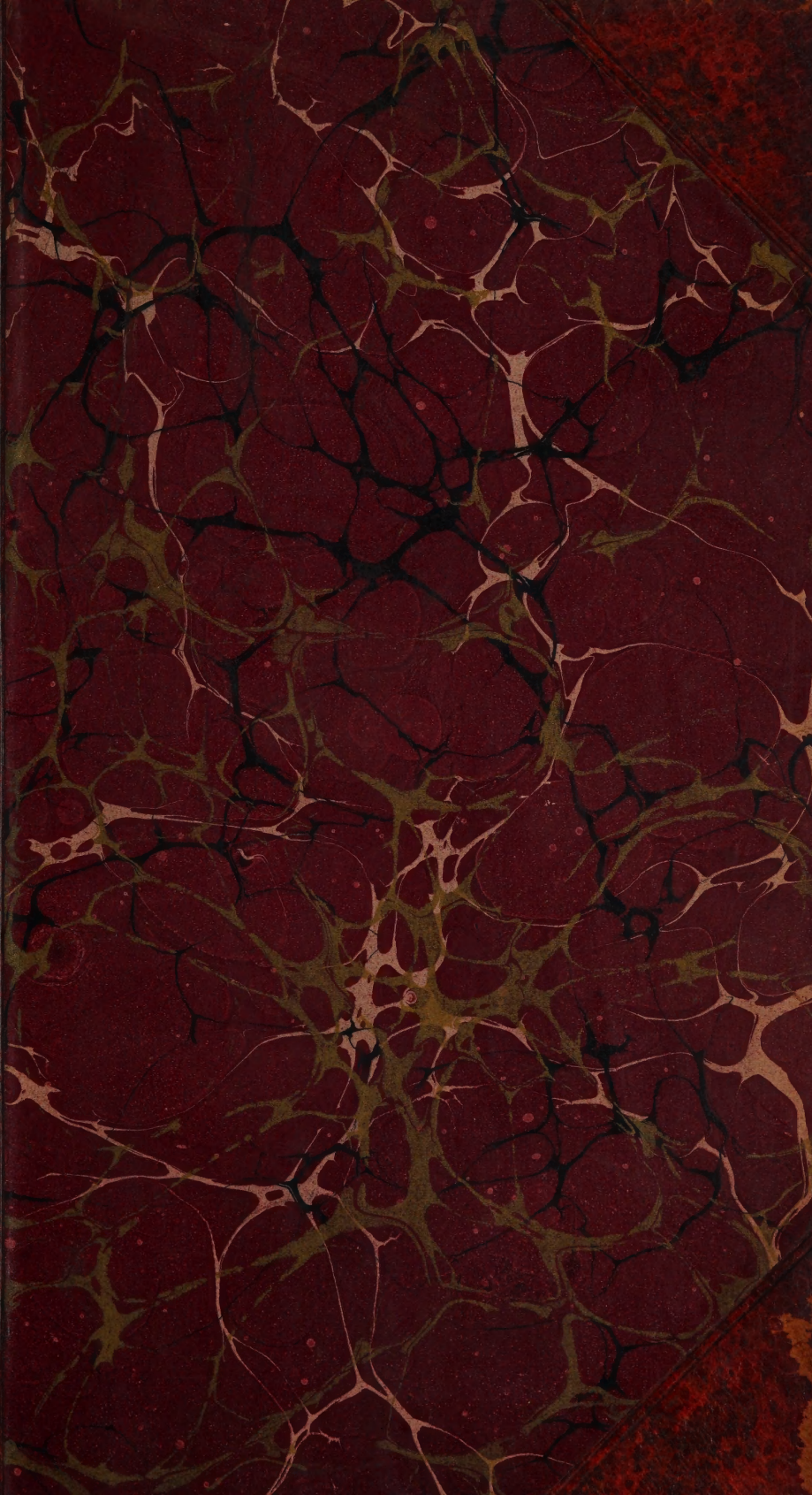


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DEVOTED TO ORIGINAL COMMUNICATIONS ON MEDICINE,
SURGERY, AND THEIR SPECIAL BRANCHES

EDITED BY

E. C. SEGUIN, M.D.

S'il est possible de perfectionner l'espèce
humaine, c'est dans la médecine qu'il faut
en chercher les moyens.

—DESCARTES



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INDEX.

- Abscess of the Left Frontal Lobe of the Cerebrum, with Special Reference to Localization . . . 107
- Anæmia in Early Childhood . . . 1
- Analysis of the Principles on which Depends the Success of the Modern Methods of Treating Wounds 264
- Aphasia, Contribution to the Study of 136
- Arthritis of the Temporo-Maxillary Articulation 259
- BALL, A. B. A Contribution to the Study of Aphasia, with Special Reference to Word-Deafness and Word-Blindness 136
- Bartholow, Roberts. A Treatise on the Practice of Medicine for the Use of Students and Practitioners. Review 86
- BERMANN, I. Some Points on Staining *in toto* and Dry Section Cutting 130
- Bryant, Thos. A Manual for the Practice of Surgery. Review. 201
- Buck, Albert H. Diagnosis and Treatment of Ear Diseases. Review 187
- Care of some classes of the Chronic Insane 28
- Case of Clonic Facial Spasm Successfully Treated by Stretching the Seventh Nerve 103
- Childhood, Anæmia of 1
- Chorea Dependent on Malaria . 221
- Chronic Insane, Care of 28
- Contribution to the Study of Aphasia, with Special Reference to Word-Deafness and Word-Blindness 136
- Croupous Pneumonia, an Acute Infectious Disease 225
- DA COSTA, J. M. The Fluorides in Medicine 253
- Endocarditis, Infectious 44
- Fournier, Alfred. Syphilis and Marriage. Review 318
- Further Note concerning Accessory Facial Foramina 113
- Fluorides in Medicine 253
- GOODWILLIE, D. H. Arthritis of the Temporo-Maxillary Articulation 259
- Hamilton, Frank H. A Practical Treatise on Fractures and Dislocations. Review 90
- Harrison, Reginald. The Surgical Disorders of the Urinary Organs. Review 199
- HEINEMAN, H. N. Two Cases of Chorea possibly Dependent on Malaria 221
- Higher Medical Education in New York 177, 291
- HOWE, JOSEPH W. Operation for the Formation of Nymphæ in the Male after Amputation of the Penis close to the Pubes 111
- Hyoscyamia as a Depresso-Motor 280
- Hyoscyamia as an Hypnotic and a Depresso-Motor 162
- Infectious (so-called Ulcerative) Endocarditis 44
- Instrument for Electro-Massage. Review 216

- JACOBI, A. Anæmia in Infancy and Early Childhood I
 ——— A Treatise on Diphtheria. Review 193
 Jamaica Dogwood, Physiological Action of 69
 JEWELL, J. S. Practical Observations on the Management of Mental and Nervous Diseases . . 115
 LEUF, A. H. P. Further Note concerning Accessory Facial Foramina 113
 Malarial Neuralgia, Case of . . 223
 Management of Mental and Nervous Diseases 115
 MENDELSON, WALTER. Case of Malarial Neuralgia 223
 Mitchell, S. Weir. Lectures on Diseases of the Nervous System, especially in Women Review 335
 Mittendorf, W. F. A Manual of Diseases of the Eye and Ear for the Use of Students and Practitioners. Review . . 333
 Modern Methods of Treating Wounds 264
 NANCREDE, CHAS. B. An Analysis of the Principles on which Depends the Success of the Modern Methods of Treating Wounds 264
 Non-Restraint in the Treatment of the Insane 79
 Operation for the Formation of Nymphæ after Amputation of the Penis close to the Pubes . . 111
 OSLER, WILLIAM. Infectious (so-called Ulcerative) Endocarditis . . 44
 Otis, F. N. Clinical Lectures on the Physiological Pathology and Treatment of Syphilis. Review 202
 OTT, ISAAC. The Physiological Action of the Active Principle of *Piscidia Erythrina* . . . 69
 Physiological Action of the Active Principle of *Piscidia Erythrina* . . 69
Piscidia Erythrina, Physiological Action of 69
 Practical Observations on the Management of Mental and Nervous Diseases 115
 PUTNAM, J. J. Case of Clonic Facial Spasm Successfully Treated by Stretching the Seventh Nerve . 103
 SANDERS, EDWARD. Croupous Pneumonia, an Acute Infectious Disease 225
 SEGUIN, E. C. Case of Abscess of the Left Frontal Lobe of the Cerebrum with Special Reference to Localization . . . 107
 ——— Report on the Use of Hyoscyamia as an Hypnotic and a Depresso-Motor 162
 ——— Higher Medical Education in New York . . . 177, 291
 ——— Hyoscyamia as a Depresso-Motor 280
 SHAW, J. C. Non-Restraint in the Treatment of the Insane . . 79
 Staining *in toto* and Dry Section Cutting 130
 STEARNS, HENRY P. The Care of some Classes of the Chronic Insane 28
 STIMSON, L. A. Traumatic Abscess of the Brain, Case of . . 218
 Stretching the Seventh Nerve for Facial Spasm 103
 Van Buren, W. H. Lectures upon Diseases of the Rectum, and the Surgery of the Lower Bowel. Review . . 323
 Wells, J. Soelberg. A Treatise on the Diseases of the Eye. Review 211
 Wood, W. C. Fever, a Study in Morbid and Normal Physiology. Review 302
 Word-Deafness and Word-Blindness 136

ARCHIVES OF MEDICINE.

Original Articles.

ANÆMIA IN INFANCY AND EARLY CHILDHOOD.*

By A. JACOBI, M. D.,

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PATHOLOGICAL processes are but the utterances of physiological functions performed under abnormal circumstances. Those functions depend on the anatomical condition of the tissues or organs. While this relation has long been established in the minds of medical men, the former, though acknowledged theoretically, is frequently not heeded. As a rule, the pathological anatomy of a diseased organ is stated, in connection with the history of a case, or the description of a class of cases, but the reference of an anatomical predisposition of tissues or organs to special morbid processes is mostly neglected. It is mainly Beneke who has studied disease from this point of view, and it is from his various essays and works on kindred subjects that some of the exact data to be laid before you are taken.

By rights, every treatise, essay or paper on a pathological subject ought to commence with the normal anatomical condition of the organ or tissue to be dealt with. Thus,

* Read before the Med. Society of the County of New York, Dec. 27, 1880.

only, an intelligent appreciation of the facts becomes possible, and thus, only, when every case is viewed in this light, the practice of a medical man is raised above the level of routine and drudgery.

When some time ago, Mr. President, I had the honor of reading before our Society a paper on infant diarrhœa and dysentery, I emphasized the fact that healthy infants have a normal tendency to loose liquid or semi-fluid evacuations from the bowels. The causes I stated to lie partly in the conditions of the intestinal tract, and partly in the nature of the normal food, viz., breast-milk. The latter do not concern us now, but the former I repeat merely for the purpose of establishing, in a few examples, the close connection between anatomical structure and physiological and pathological conditions. The peristaltic movements in the infantile intestine are very active; the young blood-vessels very permeable; the transformation of surface cells is very rapid. The peripheric nerves lie very superficially, more so than in the adult, whose mucous membranes and submucous tissues have undergone thickening by both normal development and morbid processes. In the infant, the peripheric ends of the nerves are larger in proportion than in the adult, the anterior horns of the nerve centres more developed than the posterior ones. Thus, the greater reflex irritability of the young, under intestinal and other influences, is easily explained. Besides, the action of the sphincter ani is not quite powerful, the fæces are not retarded in the colon and rectum, and no time is afforded for the re-absorption of the liquid or dissolved constituents of the fæces. Moreover, the frequent occurrence of acids, sometimes in normal conditions, in the small intestines, gives rise to the formation of alkaline salts with purgative properties.

On the other hand, constipation in the very young is sometimes the result of grossly anatomical conditions of

the intestinal tract. I should not have to allude to the fact at the present time if it were not for the following reasons: Firstly, this form of constipation illustrates exceedingly well the connection between anatomy and function; secondly, the routine treatment of constipation by the administration of purgatives would be very dangerous in just such a case; and lastly, what I have published about the subject more than ten years ago, and repeated in the treatise on hygiene, edited by Dr. A. Buck, appears not to have been noticed to such an extent that the suffering infants can be sufficiently benefited. At least, in an essay on constipation, published but lately and presumably considered complete in its etiology, this important cause of the most obstinate form of constipation in the very young is not mentioned at all.

It therefore bears repetition; it is, in a few words, as follows: Until the fourth or fifth months of foetal life, there is no colon ascendens, and it is still short at birth. Notwithstanding that fact, the large intestine at birth is comparatively longer than that of the adult. While in the infant it is nearly three times the length of the entire body, it is but twice that length in the adult. Now, the colon ascendens is very short in the newly-born, the transverse colon is not much longer; thus, the main part of the excessive length belongs to the colon descendens, and mainly to the sigmoid flexure, which Brandt found from fourteen to twenty centimetres, and myself in one case thirty cm. in length. This exorbitant length of the sigmoid flexure at the entrance of the narrow pelvis, gives rise to more than the simple curve found in the adult. Not infrequently the main curve is found on the right side instead of the left, and sometimes the repeated bending upon itself of the elongated gut is such as to seriously retard, and in a few instances, prevent, the passage of fæces.

The two instances hitherto spoken of illustrate the close connection of two conditions noticed in very early life depending upon the anatomical structure of the affected organ. In brief, I shall allude to two others which become manifest at a little later period of infant life. Thus, in rachitis, while the heart is of average size, the arteries are abnormally large, the liver is of extraordinary volume, and the lungs are small. Great width of arteries lowers the pressure of the blood. One of the results of this physiological fact is the murmur audible in the brain of rachiticial babies, which, by no means, as Jurasz explained it, results from the anomalies of the carotic canal. Another result of the low blood pressure is the retardation of the circulation in the muscles, and more yet about the epiphyses, which swell and soften. It is not the growth of the epiphyses alone which, by itself, results in general rachitis, for the epiphyses are still in their cartilaginous condition up to adolescence, and some do not ossify until the twentieth year of life; but no rachitis is met with at this advanced stage. Thus it is by no means the anatomical condition of the cartilaginous tissue which is one of the causes of rachitis, but the condition of the arteries supplying the epiphyses. Besides, the large size and active condition of the liver give rise to a copious formation of cholesterin, the importance of which, in the establishment of a hyperplastic condition of cartilage cells and tissue elements in general, has long been recognized. Thus, ossification becomes irregular and defective, and the rachiticial bone contains an abnormally large quantity of fat, in contrast with the deficient percentage of lime, which either is not introduced or not assimilated in consequence of the faulty nature of the preliminary stages of osseous development.

Some other peculiarities are found in the condition which has been called *scrofula*. The normal relation of the heart

to the lungs, between the second and twentieth years, is 1:5-7; in scrofula it is 1:8-10. This circumstance, coupled with an acquired debility of the nervous system, results in an insufficient supply of blood to both lungs and organism, and defective oxygenation, particularly in those cases which by common consent have been called torpid scrofula. It is mainly in these that the lymphatic system preëminently participates in the symptoms. The size and number of the lymphatics are very great in infancy. Sappey found that they could be more easily injected in the child than in the adult, and the intercommunication between them and the general system is more marked at that than any other period of life. These facts are but lately verified by S. L. Schenck who, moreover, found the network of the lymphatics in the skin of the newly-born endowed with open stomata, through which the lymph-ducts can communicate with the neighboring tissues and cells, and *vice versa* (Jacobi, "Treat. on Diphth.," p. 31).

The blood of the newly-born differs greatly from that of the infant at a period but little advanced. The hæmoglobin in the umbilical artery amounts to 22.2 per cent. of the whole solid constituents, while in the venous blood of the mother it is but 13.99 per cent. The first to prove this high percentage was Denis, in 1830, who found the correct proportions by determining the quantity of iron contained therein. Poggiale found a similar proportion of the hæmoglobin in the new-born and the fully-grown dog, viz.: 16.5: 12.6 per cent., and Wiskeman's results are similar. The total amount of the blood contained in the newly-born is, however, smaller than in the adult. The relation of its weight to the total weight of the body being in the former, 1:19.5; in the latter, 1:13.

These conditions, however, are being changed soon. The high percentage of hæmoglobin commences to decrease

instantly. Young animals have less than old ones; in the calf and oxen the proportion is 11.13 : 13.21.

Denis found it to diminish until the age of six months, and a very slow increase up to the thirtieth year. Leichtenstern found the following proportions: if the blood of the newly-born contains hæmoglobulin 100, that of a child of from six months to five years contains 55; of from five to fifteen years 58. At the age of from fifteen to twenty-five it is 64, 25-45=72, and 45-60 it is 63. Subotin also found less in young animals than in old ones; also less when the amount of nitrogenous food was reduced. Leichtenstern found the percentage of hæmoglobulin to decrease in the very first two weeks. It was lowest at the age of from six months to six years; after that time a slow increase takes place. But even in the very vigor of life, in the third and fourth decennia, the percentage of hæmoglobulin is smaller than in the newly-born.

There are some more differences in the composition of the blood of the young, more or less essential in character. The foetal blood and that of the newly-born contains but little fibrine, but vigorous respiration works great changes in that respect. Nasse found the blood of young animals to coagulate but slowly. How this is in the infant cannot be determined until more and better observations will have been made. There are less salts in the blood of the young, and according to Moleschott, more leucocytes. Its specific gravity in the young is 1045-1049; in the adult, 1055. Thus, letting alone the newly-born, the result from the above figures is this: The infant and child has and requires more blood in proportion to its entire weight, but this blood has less fibrine, less salts, less hæmoglobulin, less soluble albumen, more white blood corpuscles, and less specific gravity.

The large arteries in the newly-born and the infant are

wide, and consequently the blood pressure is but low. This is mainly so in the first five years, in the subclavian and common carotid. Thus the brain has a chance to grow from 400 grammes to 800 in one year; after that period its growth becomes less. At seven, boys have brains of 1100, girls of 1000 grammes. In more advanced life its weight is relatively less; 1424 in the male, and 1272 in the female. At the same early period the whole body grows in both length and weight. The original 50 cm. of the newly-born increase up to 110 with the seventh year; the greatest increase after that time amounting to 60 (in the female, 50) centimetres only. In the same time the weight increases from 3.2 kilo. to 20.16 in the boy; from 2.9 to 18.45 in the girl; a proportion of 1 to 6 or 7, while after that time the increase is but three- or four-fold.

As the organs grow, so do the peripherous blood-vessels. Their size is in proportion to the large blood-vessels. Only the heart grows toward the seventh year, perhaps, only because it requires an over-exertion to overcome the sluggishness in the circulation of the large and small blood-vessels. It is smallest, with large arteries, in the first year (particularly in the second half) at the same time that the growth is most intense. Thus it appears that the growth and physiologically low blood pressure go hand in hand.

The sizes of the large blood-vessels do not grow equally, nor do they exhibit the relative proportions to each other of the normal development of the adult. The pulmonary artery is from two to four centimetres larger than the descending aorta. That means for the lungs more active work, but also more tendency to disease, particularly as, since the closure of the ductus Botalli, the aorta, from which the bronchial arteries are sent off, assumes considerable proportions within a short space of time.

At this time the lungs begin to rival the liver, which in

the first days of life was twice as large as both lungs combined. At this time, the amount of carbonic acid eliminated by the lungs is increasing steadily to relative proportions not known in the adult, in the same manner as the amount of urea eliminated is relatively larger than in the adult, in consequence of the size of the kidneys, which are proportionately larger than in the adult.

Water prevails in the organs, even to a greater extent than the smaller specific gravity of the blood appears to justify. The brain in all its parts, but not equally in all, contains a high percentage of water, the exact figures of which can be found in "Buck's Hygiene," 1st vol., p. 139. The muscular tissue has a percentage of 81.8 (E. Bischof) in the newly-born; of 78.7 in the adult. Schlossberger found the following figures: in a calf of four weeks, 79.7; the grown-up animal, 77; the young duck, 85.4; the old, 72.

The labor required of both heart and lungs is greater than in the adult; thus fatigue is more easily experienced, and the necessity of sleep, the interruption or absence of which adds to the exhaustion and waste, is readily explained. More physiological work is done by these two organs, and, moreover, in a manner somewhat different from what we notice in the fully developed individual. In him, nothing is required but the sustenance, or rather, constant reproduction of the bulk of the body; in the child, not only reproduction, but a new development of tissues, a constant growth, must go on.

Within one year after birth, the young creature attains three times its original weight. Thus we have to deal with a being whose organs are in constant exertion, or almost over-exertion. Now, metamorphosis of matter is not controlled by the inhaled oxygen alone, for the living organism is not only what Liebig took it to be, an oven; its in-

tensity depends certainly in part on nerve influences. As the nerve cells contain so much more water than in later periods of life, it is very probable that their electro-motor action differs from that exhibited later on. Besides, the predominating development of the medulla oblongata, the anterior horns and trophic nerves, points to the same conclusions. All this action and activity is at the expense of the system. But that is not all. Not only exertion and almost over-exertion, when compared with the efforts of the merely self-sustaining adult system, but constant production of new material, and all this at the expense of a blood which contains less solid constituents than the blood of the old. Thus the normal oligæmia of the child is in constant danger of increasing from normal physiological processes. The work before a baby has to be performed, under the most favorable circumstances, with, so to speak, a scarcely sufficient capital. The slightest mishap reduces the equilibrium between that capital and the labor to be performed, and the chances for the diminution of the amount of blood in possession of the child are very frequent indeed.

Thus, the vulnerability of the young being great, and diseases in early infancy and childhood so very frequent, cases of anæmia are met with in every day's practice, and in every form, complicated and uncomplicated, with great emaciation or without it, and either curable or not. A condition so frequent, so variable, so dangerous, deserved to be treated in monographs by the best men amongst practitioners and writers, and still there is scarcely any text-book, any journal, in which a competent and comprehensive view of the subject can be found. There is but one noteworthy exception to this fact. Dr. Förster, of Dresden, contributed two years ago a valuable essay on the subject in one of the most praiseworthy literary under-

takings of modern medical authorship. There are two great works in pædiatric literature recognizable as landmarks. The first were the three volumes of monographs published by Rilliet and Barthez. The second is the great manual on diseases of children, edited by C. Gerhardt. In its third volume, Dr. Förster's article has been published. Like others before him, he makes a distinction between idiopathic and symptomatic anæmia.

The former diagnosis is made when there is no tangible cause at all, or none which still persists; the latter when the change in the blood, with all its consequences, is attributable to a previous or present sickness. Perhaps it is idle to consider the question at all, whether there can be a genuine, primary idiopathic anæmia. When we sift the matter, we shall come to the simple conclusion that everything has its cause, is but a result, and secondary to something else. From this point of view, and strictly speaking, objection could be raised to the term of idiopathic pneumonia, peritonitis, or meningitis. When we make use of it, we mean to state only that the local affection is no longer complicated with any other that could be diagnosed, and, possibly, removed.

In this sense there are cases of idiopathic anæmia, in which the original infant disposition to it, of physiological character, has been raised to a pathological dignity. But the large majority of cases are of markedly secondary character, and cannot be appreciated or treated rationally without the recognition of the original causes. They are of the most various character. In fact every disease occurring in infancy and childhood may give rise to anæmia. Very few diseases when they have run their full course and terminated in what we are pleased to call recovery, leave the organism or the affected organ, in as perfectly a normal condition as previously. The frequent recurrence of simple diseases such

as pneumonia points to the fact that changes have been worked which create a constant predisposition to pathological processes in the same organ. Thus, in most cases of anæmia the diagnosis of the whole case must extend to the organ first affected, and the treatment, while it may be directed against the result, is incomplete unless the causal indications be fulfilled.

Hemorrhages result in anæmia in a number of instances. They are of different character and importance. There is true melæna ; umbilical hemorrhage ; hemophilia ; primary or secondary purpura ; internal hemorrhages of the newly-born ; cephalhæmatoma ; hemorrhages from rectal polypi ; epistaxis depending on coryza ; epistaxis at a more advanced age from heart disease and abdominal stagnation ; hemorrhages in diphtheritic angina ; and such as take place during or in consequence of operations for hare-lip or ritual circumcision. Death may result from many of them, such as melæna, hemophilia, pharyngeal hemorrhages, or circumcision ; others are of but little gravity, such as the sanguineous tumor of the newly-born ; others are apt to result in permanent ailing. As a rule, however, an acute anæmia is more easily overcome than one that is of a more chronic nature, and thereby undermines the vitality and strength of the organs while it slowly robs them of their nutriment. Infants who are thus stricken recover but slowly or not at all. Young animals resist starvation to a less degree than old ones. A dog of two days bore starvation in Magendie's laboratory but two days ; a dog of six years, thirty. Similar results were obtained by Chossat in his experiments on pigeons. Thoroughly anæmic and delicate babies seldom recover entirely, like starving young animals which never attained their normal condition though they were carefully fed afterward. The recruits of the Prussian army born in the starvation years of 1816 and 1817 were of a very inferior

character, physically. To this class also belong the children born prematurely and of delicate parentage, though there were no recognizable constitutional disease, and of mothers afflicted with a disease of the uterus or placenta, inflammatory, syphilitic, or otherwise; or of such as suffered much during pregnancy or lactation; also those born with congenital diseases, cyanosis, or neoplasms, which are by no means so rare as has often been believed and said, or the peculiar smallness of the heart, and principally the arteries, to which Virchow attributes many cases of chlorosis. I have met with half a dozen of such cases, in which the supply of blood to the body was diminished by this anomaly, and Dr. Skene reported a case of probably the same nature which was published in the *Journal of Obstetrics and Diseases of Women and Children*, Oct., 1876.

Besides the diseases and affections of the newly-born there are others which develop in later life and lead to the same results. It is often acquired in endocarditis, for instance; acute inflammatory rheumatism, which is very frequent, yields in most cases but little swelling of the joints, comprises most cases of so-called growing pain, and has a much more marked tendency to the production of an endocarditis than the same affection in the adult.

Protracted diarrhœa injures to a greater extent than constipation. It acts not only by the direct and immediate loss of serum, through which it can prove fatal in a short time, but more frequently by its consequences. The mucous membrane of the intestinal tract becomes thickened, the submucous tissue œdematous, the muscular layer œdematous or hyperplastic; the adventitia sometimes undergoes fatty degeneration. Erosions and ulcerations are apt to become chronic, and frequently the mesenteric glands are the seats of congestive and hyperplastic processes. An intestinal catarrh cannot last any

length of time without irritating, congesting, enlarging, and finally indurating, or provoking caseous degeneration of the neighboring lymphatic glands. The cause of the diarrhœa is indifferent in this respect. None can last without consecutive injury to the lymphatic glands which is apt to become permanent and deteriorate sanguification for the future. The unmistakable practical conclusion from this fact is that every diarrhœa must be stopped as soon as possible. Neither summer heat, nor that great scape-goat—dentition—must be permitted to yield a pretext for the continuation of a diarrhœa, no matter how innocent it may appear.

Malaria, which is too often diagnosticated when the real nature of the disease is not recognized, and frequently overlooked because of the irregularity and the little pronounced character of the attacks. The first stage of the attack is often not recognizable. The attacks are apt to come at irregular times; are more quotidian than tertian, often concealed by accompanying symptoms such as convulsions, and, therefore, sometimes not accessible to a ready diagnosis. On the other hand, the influence of malaria is apt to undermine the general health, render the child intensely anæmic, and swell the spleen considerably before ever giving rise to a real attack.

Nephritis, with albuminuria, not the acute cases, but those chronic ones which slowly undermine the nervous system and exhaust by direct loss; pernicious anæmia, with, it is true, as far as I know, but two cases occurring in children, recorded in the literature of this recent subject; leucocythæmia; sleeplessness from any cause such as malaria, hooping-cough, or indigestion; mercurial cachexia, rare though it be; congenital or hereditary syphilis; rhachitis, with its influence on blood, glands and bones, its shortening, flattening and even retraction of the thorax, its curvature of the

spine, and compression of the lungs and heart; fatty liver; enlargement of the lymphatic glands, mesenteric, bronchial or otherwise; the complex of symptoms comprehended under the general head of scrofula; diseases of the bones of the most various kinds, from the congenital or premature ossification of the costal cartilages, with its consecutive contraction of the chest and compression of its contents, to the chronic or subacute osteitis of the vertebral column or any of the other parts of the skeleton, with its final termination in amyloid degeneration of the viscera; and finally, to conclude with, diseases of the lungs and pleuræ, caseous deposits, cirrhotic induration, emphysema and empyematic deformity.

In anæmia both the skin and the mucous membranes are pale, of a yellowish hue, thin and flabby. A certain degree of apparent elasticity of the skin and subcutaneous tissue is noticed only in cases of œdematous effusion. Those organs or tissues which are least in use emaciate first; that is, in very young children, fat and muscle. But there are cases in which fat is persistently retained, and in which it is often increased in quantity. For, when the red blood globules are destroyed, there is scarcity of oxygen, and for that reason the combustion of the albuminous substances becomes incomplete, and fat, the physiological result of this incomplete combustion, is deposited in large masses. Particularly is this the case when anæmia is either complicated with or is the result of general rhachitis—when at the same time the glands and the chest are suffering from the results of the rhachitic processes. An illustration of this peculiar occurrence, which is by no means rare, is also seen in the peculiar appearance of acardiac or acephalic monsters, which contain a large amount of œdematous fat, in consequence of the exclusively venous character of their circulation.

In consequence of the ill nutrition and the emaciation of the muscular tissue these infants and children are easily fatigued. In general, the functions of all the organs suffer considerably. And with such debility, irritability goes hand-in-hand. The nervous system is less affected than any other, because of the rapid growth and development which it undergoes at that period of life. Not infrequently, babies who are anæmic and emaciated are in the very best of spirits, because their brains are comparatively in good condition. A certain amount of emaciation can be easily recognized by the depression of the fontanelles of babies under one year old or even later; but the emaciation of the brain does not increase at a rate which corresponds with the loss in weight of the other organs and tissues of the body. In addition, the very sinking in of the fontanelles, which allows us to estimate the amount of emaciation that has taken place inside of the cranial cavity, leads us to the fair conclusion that the emaciation of the rest of the body has taken place to an unusual extent; and any baby with considerable depression of the fontanelles must be considered in danger from the degree of inanition present.

Murmurs in the jugular veins are not very frequent in infancy and early childhood. Murmurs in the carotids and over the large fontanelles, however, are not at all rare. It is not true that these murmurs, audible over the brain, belong to rhachitis alone. They are found in every condition in which blood pressure in the large arteries of the cranial cavity is lessened.

The heart itself seldom exhibits functional murmurs. Whenever they are present, it is safer to attribute them to organic disease than to merely functional disorder. Besides, it is now well known that acquired endocarditis is by no means rare, and, moreover, that it occurs even more fre-

quently in the articular rheumatism of the young, be it ever so slight, than of the adult. Although the brain be not so liable to suffer from emaciation, dependent upon anæmia, as other organs, still there are a number of cases in which headaches, attacks of syncope, sleepiness, etc., or, on the contrary, sleeplessness and hysterical attacks, are the result of anæmia alone, and disappear when this condition is relieved. Not a few of the babies and children who cry the greater part of the night have no other ailment besides general anæmia, and such children are frequently relieved by a meal or some stimulant before they are put to bed, or given during the interruption of their sleep. The pulse of such children is sometimes very much accelerated; sometimes, however, it is slow, and sometimes irregular. I have known such children, in whom for months, and occasionally for years, I have feared the development of cerebral affections from the very fact that their pulse was both slow and weak; and yet, when their general condition was improved, both the regularity and frequency of the pulse were increased.

The pulse, however, is, perhaps, amongst the symptoms which are most unreliable at this age. In the baby it is best counted during sleep, and better over the fontanelle than upon the radial artery. It will change very frequently, not only with alternate sleeping and waking, with rest and restlessness, but sometimes without apparent provocation. A slight amount of muscular action will change its character more or less, and frequently considerably. Physiologically, the pulse is very apt to be more frequent at the age of two and a half or three months than earlier or later, because it is at about that age that muscular movements are actually developed.

Very few anæmic children have a good appetite except at the beginning. The influence of anæmia is general in

regard to all organs of the body. Circulation is deficient, and the normal secretions are defective or deficient in consequence. That is, both appetite and digestion are impaired, and sometimes destroyed, and cannot be restored until the general condition of the child is improved.

The slowness of the circulation and its insufficiency, and the watery condition of the blood, are apt to give rise to catarrh of the pharynx and larynx and the respiratory organs in general. Besides, the walls of the blood-vessels are known to suffer in anæmia. They become thin, and undergo fatty degeneration, which Ponfick has found in the heart, and in the intima of the larger blood-vessels and in the capillaries. In consequence of the thinness of the blood and the changed condition of the blood-vessels, serous transudation, and, now and then, extravasations will take place. The same occurrence is noticed in the adult in conditions of anæmia. It not infrequently occurs that those who have least blood lose it most easily. Anæmic women are very apt to have copious menstruation, and when their general condition has been improved, both blood and blood-vessels resist this tendency to hemorrhage.

There is one consequence of the anæmic condition which is of the utmost importance, and requires urgently that it should be removed in the shortest possible time.

Whenever a disease sets in it is more liable to result fatally in consequence of impaired powers of resistance, and where there is the slightest tendency to effusion or to exudation these processes will become more extensive and dangerous in less time than in the normal organism. A pneumonia, a peritonitis, a pleurisy, occurring in an anæmic child, is attended with a great deal more danger than when either of these affections occurs in a child enjoying good general health.

That epistaxis in a child 5, 6, or 8 years old should last

as long as the patient is in a generally impaired condition, is just as frequent an occurrence as it is a common experience to meet with almost constant improvement after a change of diet, change of air, and a few doses of iron.

The predisposition to anæmia in the child is very great, as proved before, and the causes of its development very numerous. These causes must be, according to circumstances, either prevented or remedied. For genuine cases of idiopathic anæmia are certainly very rare, and an accurate diagnosis will find it to be symptomatic in almost every case, and to depend on the lesion of some organ, or system of organs. The danger of anæmia is greatest at the time of the most rapid growth, still it is a cause of slow destruction in every age. The nursling is more exposed than the child, for the growth of all the organs, with very few exceptions, is most intense at the earliest period of life. At that time, besides actual disease, insufficient food, or improper food, are frequent causes, the latter a more frequent one than the former, and often the more dangerous one of the two. Infants whose mothers or nurses have not enough milk, simply starve; they lose weight, strength and color. As long as their lungs and muscles will hold out, they will scream. Some of the yelling heard in the night amongst the tenement-house population, and sometimes in the better-situated classes, too, comes from starving babies. After a while the yelling turns into a whining, and any slight disease terminates the baby's suffering. This condition is recognized by the absence of local disease anywhere, by the gradual emaciation, and is characterized by the paucity of otherwise normal fæces. Many a case of alleged constipation is one of starvation. Where there was no food, there are no evacuations, and when a baby is reported as having but one normal passage a day, or even less, the suspicion is that it has

not enough to eat. The remedy is easily recommended, for it consists in nothing but a sufficient quantity of proper food.

Improper food is a much more frequent cause. A few remarks must suffice here, for it is impossible to go over the whole ground of infant hygiene in a short paper which is more meant to suggest than to teach. A few points, however, I must not omit, because of the frequency of the sins committed. The contra-indications to a woman's nursing a baby must be obeyed. Nursing during pregnancy, or extended over too protracted a period, must be forbidden. The latter is, if possible, more serious than the former. Many a case of rhachitis or anæmia owes its origin to the baby being nursed into the second year. A baby whose development is not normal, for instance, whose first tooth does not appear at the regular age of seven or eight months, is either suffering from a previous disease or it has insufficient or improper food. If nursed, therefore, it ought to be weaned, or partially so. Many a flabby child at the breast will thrive when weaned at last, and good barley and cow's milk will make better muscle and teeth than poor mother's milk. An inherited or inheritable or communicable disease on the part of the mother or wet-nurse, such as consumption, rickets, syphilis, serious nervous diseases, intense anæmia, forbid nursing. In not a few cases the individual milk of mother or wet-nurse does not agree with the baby. When such is the case, unless the fault can be detected and remedied, weaning is required. In most cases it is possible to trace the indigestibility and insufficiency of a mother's milk to the absence or prevalence of a special constituent, mostly either sugar, or—and mainly so—casein. A beautiful illustration of this fact was but lately exhibited by a baby patient of Dr. A. N. Smith. The mother's milk was undoubtedly too white

and too caseinous. The baby's digestion was faulty, his assimilation quite defective. The addition of some farinaceous decoction to each meal from his mother's breast—a few teaspoonfuls given before each meal—remedied the evil somewhat, but the patient's life was finally saved by nothing but weaning and exclusive artificial feeding. It is impossible, however, to consider now the question of infant food to any extent. Such principles as I have laid down in Buck's Hygiene, and very briefly in my paper on infant diarrhœa and dysentery, have guided me through the better part of my life. I shall not, therefore, tire your patience by repeating them. There are, however, a few simple words which I cannot repeat too often. Avoid solid food in the care of an infant. Avoid cow's milk either undiluted or diluted with water only. Avoid condensed milk diluted with water only. Use no milk without the addition of some gelatinous or farinaceous decoction, barley, oatmeal, gum arabic, gelatine. In anæmia, add beef soup to the uniform infant food daily. Give solid food, that is a small piece of meat, a crust of bread, half an egg, about the end of the first year. Keep up this simple diet for another year, and add slowly such articles of food as physiology and experience permit. Prohibit bad habits, such as irregular and fast eating, cold feet and highland-fashion legs, and enforce out-door exercise; children before and after an out-door play are different beings. Avoid crowded school-rooms and the excess of private lessons. A child sleeping after a healthy exercise of his muscles and lungs will finally, besides being stronger and healthier, learn more than one who hangs his pale cheeks, sleeping over his books. We have laws to protect children from being sent to work in factories, or to be employed on the stage, but we have none to protect them from the equally destructive, incessant schooling in close

rooms, without air or exercise. There are too many books bought for Christmas and too few skates.

Amongst the medicinal agents iron has long been the main resort in anæmia and chlorosis. This was so even before the time when hemoglobin was isolated and found to contain all the iron of the blood. As it was found to benefit the cases of anæmia and chlorosis, in which the red blood corpuscles were undoubtedly diminished, it was believed that iron had the ability to directly increase the number and the quality of the red blood globules. But the question whether it is really the iron which produces this effect has not been answered to the satisfaction of all, for a great many of the cases get well while no iron whatever is given, and in consequence of change of diet and the securing of rest and a better general condition. Besides, there are a number of cases in which the administration of iron is absolutely unavailing. Moreover, there is plenty of iron in almost every article of food. Boussaingault found that thus eight or nine centigrammes (gr. iss) of iron are daily taken into the body. The same quantity has been found by Fleitmann to be eliminated by the kidneys and the intestinal canal. Thus, there certainly are cases of chlorosis which have not been caused by the absence of iron; and it cannot, therefore, be said that the iron, by supplying this lack or by removing this absence, cures chlorosis.

But it is still a question whether the iron thus given, under circumstances which are entirely abnormal, does not improve the chances of recovery in just these conditions. The doses given would certainly be too large, when compared with the iron contained in the food and with the amount of iron present in the whole quantity of circulating blood, three grammes and no more.

Compared with this small quantity, the doses we are accustomed to administer are certainly large. Speedy elimi-

nation, too, takes place, through which the whole or nearly the total amount of the ingested iron is removed. But it has not been found whether the iron does not act in some other way besides increasing the amount of the metal contained in the hemoglobin.

After iron has reached the stomach it is decomposed into an oxide, and is absorbed, probably in the form of an albuminate. There can be no doubt, according to Dietl and Heidler, that it is absorbed in the stomach, and very probably the upper part of the small intestine also. It reappears in the bile and the pancreatic juice. Not only is that the case after it has been introduced into the stomach, but it will also reappear in the bile secretions of the intestine and pancreatic juice, according to A. Mayer, after it has been injected into the veins. It is true that Quincke was sometimes unable to find iron in the intestinal secretions after it had been injected into the blood, but it seems to be well established, according to the experiments of Prokowski, that the temperature of the blood is elevated, the pulse accelerated, and the blood pressure increased after the use of iron. For this reason it ought not to be given during the height, or even during the course of inflammatory fevers. A number of its preparations are certainly vascular excitants. But for this very reason, while it is contraindicated in inflammatory fevers, it certainly is indicated and required in most cases of septic fevers.

The preparations most beneficial in anæmia of children are, in my opinion, the following: the lactate, the tincture of the pomate, the iodide, the pyrophosphate, the subcarbonate, and the tincture of the chloride.

The lactate and the pomate are very digestible, and may be given whenever the indication for the use of some mild preparation of iron is established.

The syrup of the iodide has an advantage over the other

preparations of iron, because by its use two indications may be met—that is, where the additional aid of an absorbent is desired. Therefore, it is the proper remedy in cases of slow convalescence after inflammations resulting in exudation, particularly in disease of the glands and the lungs. It has, moreover, one peculiarity which makes it much more desirable than many other preparations, and that is, it is easily decomposed in the stomach; the iodine is set free, and acts as an antifermentative in the many cases of disturbed gastric digestion occurring even in normal children, and almost certain to take place in children whose circulation has been disturbed or whose gastric secretions are certainly below their normal amount in consequence of a deficient supply of blood.

The subcarbonate of iron is a very mild preparation, easily digested, and, properly combined with a number of drugs, such as bismuth or bicarbonate of soda, is of considerable value when, in slow convalescence or progressive anæmia, this gastric catarrh threatens to interfere with the improvement in the general condition. The doses may be larger than those of any of the other preparations. A child two years old will easily bear from 25 to 50 centigrammes daily. This quantity, combined with twice or three times as much subcarbonate of bismuth, and, if necessary, three or four times that amount of bicarbonate of soda, is a very proper remedy to be used in the conditions alluded to.

The tincture of the chloride of iron, when neutral, is a preparation which is also easily digested. Doses of a gramme daily, or more, are very readily digested, and prove beneficial. This can be easily combined with the bitter tinctures, stomachics, etc. The tincture of the muriate of iron is the one, amongst the ferruginous preparations, with the exception of those partly composed of ether, the acetate, for instance, which must be regarded as a vascular

irritant, and wherever the action of the heart is lowered and blood pressure is diminished, it is the preparation which will be found most beneficial.

In a number of cases, the choice among the several preparations of iron is an indifferent matter, at least, so it appears to be. Still it has seemed to me that, in those cases in which I have had to deal with anæmia attended by gastric catarrh and digestive incompetency in the upper portion of the small intestine, the pyrophosphate proved very satisfactory. I have employed the compound hypophosphites and phosphates a great deal, which combine iron, potassa, lime and soda, and, although it is well known that the elimination of these metals and metalloids is almost as rapid as their ingestion, still it appears that the effect produced by such combinations is a very happy one in just such conditions as those of which we have just spoken.

All these preparations are of special value in chronic anæmia, which is by far the most common affection. Acute impoverishment of the blood, such as that caused by severe puerperal hemorrhage or hemorrhage from the bowels, is fortunately very rare in infancy and childhood. Therefore, the opportunity for transfusion of human blood is seldom offered, even to those who are most fond of that particular operation.

The doubtful results of transfusion upon a large scale have induced a modern writer to make a number of small transfusions by means of the hypodermic syringe. He would withdraw blood from the vein of a healthy person and introduce it directly and immediately into the veins of the sick child, and he states that he has done so with favorable results. It seems to me that the plan is rational enough, but the future must decide whether the results will be as favorable as they have been reported, and whether

there will not be grave objections to what is described as a very trifling operation. If it be successful, it would certainly, under equal circumstances, have the preference over the slow process of gastric, or of rectal alimentation, no matter whether injections of defibrinated blood or other nutrients are used.

In cases of chronic anæmia I have frequently used arsenic ; one or two minute doses daily, after meals and well diluted with water, and with benefit. Of one thing there is no doubt, and that is that arsenic does good in a peculiar torpid condition of the stomach which will not digest and assimilate in consequence of the absence of both nerve power and gastric juice. Both in adults and in children, I have given it for the purpose of improving general nutrition, and I have not seen in children what very frequently occurs in adults when arsenic is given for nervous disorders, namely, gastric derangement. With iron, with or without stomachics, I have seen the appetite improving, the mucous membrane filling with blood, and vigor returning under its restorative influence. Doses: from two to five drops daily, of Fowler's solution.

In this connection, I will state that strychnia, in my hands, has proved very beneficial as an adjuvant to either arsenic or iron. To a child two years old a dose of $\frac{1}{40}$ of a grain may be safely given daily, and this dose may be continued for a long time. Its action is well known in cases in which the digestion and the entire nervous power of the patient are simply lowered, and a few weeks' administration, together with proper food and either iron or arsenic, has changed the condition of the anæmic child considerably.

Phosphorus, in about the same doses as strychnia, has also produced very happy effects. They may be brought about by the influence of phosphorus upon the nervous

system, or they may be explained by the effect which the remedy produces when given in diseases of the bones. Some ten years ago, Georg Wegner found that the fractured bones of rabbits fed upon minute doses of phosphorus, would unite much more rapidly than the fractured bones of those animals which were left to themselves. Since that time I have been in the habit of giving phosphorus in cases of acute and chronic disease of the bones of an inflammatory character, and in caries particularly, and my impression is that the large majority of cases do very much better when small doses of phosphorus, say $\frac{1}{160}$ to $\frac{1}{100}$ of a grain daily, are given, than when the disease is left to pursue its course without the use of this remedy. It is true that the time required by such a process as caries is long under any circumstances, but it has seemed to me that even caries of the ankle joint and the metatarsus was apt to progress very favorably in the course of a number of months when phosphorus was used, whereas years were required in other cases which had not received the same treatment.

I do not know that it has been used extensively in rachitis, but it is not improbable that the good effect which phosphorus produces in anæmia, mostly of rachitic children, is partly due to the fact that the bones especially show an increased tendency to normal development.

In many cases cod-liver oil is very serviceable; I need not speak of its effect, and shall only say that frequently the contra-indications to its use are overlooked. Most children do not bear it well in the summer, when it is apt to produce either gastric catarrh or diarrhœa. Some do not bear it at all at any season of the year. It is with cod-liver oil as with any other remedy, particularly iron, of which I have already spoken. There are children

who do not bear either, and therefore they must be treated without these remedies. At all events, it should not be forgotten, whenever digestion is impaired, whenever there is gastric catarrh, that these cases require preliminary treatment before the administration of either cod-liver oil or iron is resorted to.

THE CARE OF SOME CLASSES OF THE CHRONIC INSANE.*

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“When a man loses his reason, it becomes necessary that the reason of others, in a greater or less extent, shall supply its place. To that extent, the movements of the person thus affected are subject to the control of others, and his property is taken from his management and disposal. Humanity demands this; the peace and safety of society demand it, and the ultimate good of all parties is promoted by it.”—ISAAC RAY.

GIVEN one, or three, hundred subacute or harmless chronic insane persons to be cared for in the most judicious, humane and economical manner, how shall the problem be solved?

That an intelligent answer may be furnished, it may be necessary to describe more fully the general condition and peculiarities of the above-named class of persons.

Some of them will be affected with delusions and hallucinations of a more or less pronounced character. In others the memory and judgment will be “fatally impaired,” rendering them incapable of protracted thought, and ready to misjudge even the most benevolent intentions of others. Others will be demented and comparatively inactive, without ability to support, or even to assist themselves, when

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subjected to the ordinary friction of every-day life and labor. They, therefore, will require to be cared for, rather than care for themselves; they must have some degree of supervision in reference to food, sleep, dress and cleanliness.

All these persons will be more liable to illness, especially when exposed to the usual changes of weather and climate, than persons in good mental health; and, when ill, will require more care than persons in an ordinary state of health.

Cases of a subacute or chronic character will become more or less excited or depressed at times; their delusions and hallucinations will become of a more active and serious nature, and consequently at such times those so affected would require more particular oversight both day and night; and, in proportion to the degree of excitement or delusion, will very likely resent interference on the part of others in the way of restrictions upon their plans and movements. They will be quite sure they were never in better health or more capable of caring for themselves, and, therefore, their judicious management and control will require a considerable degree of tact and experience on the part of those with whom they may be placed, to avoid disagreement and the use of force; and when, as will often be the case, it may become necessary to limit their movements and usual degree of liberty, they will be very likely to claim that they are abused and unnecessarily restricted in their desires and plans, being quite sure they were never more capable of caring for themselves.

Those suffering from depression, even of a mild form, are not unfrequently affected with delusions, and consequently would require some degree of oversight both day and night. Finally, many of these persons will present peculiarities of habit, conduct and dress, and thus will not fail to attract attention, and many times intrusion, on the part

of some portion of the public, whenever opportunity may occur.

These statements, in reference to the conditions and peculiarities of the class of persons to be cared for, are believed to be essential that the elements of the problem to be solved may be somewhat more clearly appreciated.

It is not to be inferred that any one or more of the conditions mentioned will pertain to each patient all the while, though some of them will be certain to do so at times. Persons, good-natured, quiet and contented to-day, may be quite otherwise next week; minor degrees of excitement may alternate with those of depression, while peculiarities of conduct will present themselves more or less at all times.

Now, three modes of caring for these persons will readily suggest themselves to the reader :

1. They may be retained in the asylums for the insane where many of them are at the present time.

2. They may be placed in homes or asylums specially constructed for, and adapted to the use and care of, such a class of persons.

3. They may be placed in the private homes of farmers and others in the several towns and counties of the state.

I propose to refer to and examine to some extent the above named plans in the inverse order of their mention; *i. e.*, first, the placing them in homes with farmers and others scattered about the country towns and villages.

And, first, it may be remarked in reference to this plan that it is not new. It has been and still is in operation at Gheele, Belgium; at Kennoway and Ayr, and in many private cottages throughout Scotland.

The oldest and best known example is that at Gheele, where the whole village, and indeed the whole canton, is so used. At the time of my visit there some years since, there was a regular asylum, similar in its arrangements and con-

struction to other asylums, with capacity for some two hundred patients, more or less, and officered with superintendent and assistants. To this asylum all cases are sent, and while the acute cases, of excitable and maniacal type, are retained, other cases of a mild form or depressed condition are distributed over the town in the different families, who eke out their scanty income by caring for them. Not more than two or three persons are placed in any one family, and they are visited once, twice or thrice per week as their mental and physical condition may seem to require; while in all that pertains to their moral management and care, they are left under the supervision of the family. In the other two instances referred to, in Scotland, some twenty or thirty of the quiet and demented class are placed in the cottages of the villagers, and in no case are there more than two persons in any one cottage. The patients are expected to be always considered and treated as members of the families with whom they are placed, as far as practicable. Those families are composed of persons in the humblest walks of life, in fact, are poor peasants, and live in houses such as are generally occupied by that class of persons in that country; consequently, the small sum paid for the care of such patients is an object to them.

These persons are visited by some member of the Board of Lunacy Commissioners from time to time, and I think the overseers of the county poor are also expected to have some oversight of them.

As, however, these modes of providing for some classes of the insane have been in use at Gheele for centuries, and for several years in Scotland, and there has been no movement toward extending them in either country, but rather the continuance of other modes; and, as they are generally admitted by all to be impracticable in this country, no further reference need be made to them in this place.

Not so, however, with the plan of placing patients in single houses about the country; and as this is thought by some to be practicable, it may be well to examine somewhat in detail its advantages and disadvantages.

1. The plan provides for as many additional patients of a more urgent class as may be removed from asylums where they now are, of the chronic class.

It is certainly true that asylums in most or all of our states are over-crowded, and that there are considerable numbers in their homes and in the alms-houses waiting their turns for admission and treatment in them.

2. It also provides for the removal of a number of patients from the somewhat routine life incident to asylums to the more home-like and natural mode of *family life*. It is also thought they may be to a considerable extent associated with the members of the family, be entrusted with a larger degree of liberty, and be employed in the usual avocations of those with whom they may be placed. It is thought there can be found among the farming and rural population a considerable number of "farmers and others" who will be willing to receive and care for those even of the "sub-acute" class of the insane, where they can be treated by some country physician, and live safely and even "happily," their condition being reported from time to time to the General Board of Lunacy Commissioners; that such disposition of patients by their friends would be vastly more in consonance with their sympathies and feelings than placing them in asylums, whose gloomy halls and corridors they think they can see, and whose locks they imagine they can hear grating.

Now, as preliminary to considering any disadvantages attendant on such a plan, it may be remarked that there does not now, nor, so far as I know, has there ever existed any obstacle, public or private, to making such disposition of in-

sane patients, except such as may be inherent in the nature of the case. There certainly exists no law compelling persons to send their insane relatives and friends to an asylum. They can care for them in their own homes ; they can hire their neighbors to care for them, or they can remove them to distant towns and villages, and hire them cared for there, so far as any *law* of the state is concerned ; or they may at any time remove their friends from asylums and have them thus cared for, thus avoiding the dread and suffering supposed to be incident to having them in an asylum ; but, as the case actually stands to-day, this is not done, and the vast majority of those who are able to support their insane relatives have them in asylums.

Such being the case, the plan of *boarding out* of patients must relate to that class who neither can pay for their own support, or be paid for by friends.

Now, one of the first objections to such a plan of caring for the above class of the insane which suggests itself, is that it practically does away with any efficient system of daily or weekly visits and inspection. Such a system, carefully followed up by persons duly qualified by education and free from selfish interest, is believed to be absolutely essential to their well-being and humane care. This would be essential even in the case of persons in good health, unless they should be provided with the requisite means of defending themselves against the many forms of injustice which might be brought to bear upon them through the selfish interests of those under whose care they might be placed. How much more would it be the case with persons all of whom are affected with some form of illness, which renders them helpless ; persons demented, depressed, or unnaturally excited !

If it is found difficult in many cases to secure kind and judicious treatment of such persons in asylums, with the

many means for this purpose at hand, how much more in as many homes as there are persons scattered about the country! If it is found necessary to have close observation and very frequent inspection when they are cared for by persons who have been educated and trained for this purpose, and who understand that the peculiarities and oft-times obstinacy on the part of patients are due to the presence of disease, how much more will it be the case when they are among people who have little or no qualifications for their care, and whose interests may lead them to misjudge conditions which may be present.

Not long since, a father came to the Retreat to visit his son, and when told that he refused food and had to be fed with a stomach tube, exclaimed: "Why don't you knock him down? That is the way I did before he came away from home, when he did not eat! You must not be fooled with his nonsense."

Within two years, a husband brought his wife to the Retreat, and on her person were found marks of an ox-whip which he admitted he had whipped her with because she refused to answer some questions he had put to her.

Within eighteen months a mother brought her son, a lad of seventeen years, to the Retreat, and, in giving an account of the case, said she had had him chained to the garret floor for quite a long time, because he would not attend to his work, but leave home.

In these cases the two men were farmers, and the woman was the widow of a farmer. They were all persons of property, and probably not greatly worse or better, not more ignorant or learned, humane or inhumane than many others of their class in life. They had not *intended* to do an unwise or an unjust deed, but really knew no better. They did, as thousands before them have done in all ages and

conditions of life, and will continue to do, except they be educated to a more enlightened and humane course.

It seems to me, therefore, that before placing demented, deluded, or hallucinated persons, or persons who are odd and singular in their general conduct, or dress, or language, —persons whose conduct must often render them liable to be misunderstood and misjudged, in homes where they can only be visited at considerable intervals by those having charge of them, it would be well to carefully consider the matter further.

Another objection would lie in the difficulty of finding suitable homes for them at such cost as the state will pay.

In England and Scotland, these patients, who are all of the humblest class, are placed with those in like position in the general population, and who live in a very primitive manner. They are poor, and greatly need any addition they can obtain to their usual income.

In this country, farmers and others with whom any persons could be placed are in a much higher position and have a much larger yearly income. Their time is more valuable, because wages are higher, and, as a rule, they are their own employers. Such labor as could be had from one or two chronic insane persons would be of comparatively little value, indeed, would not make it worth while to be at the necessary trouble of obtaining it. They would, therefore, in many cases, be left to themselves, to wander about the country, with all the results of such liberty attending, or else be confined in the house. Indeed, under almost any circumstances, they must be neglected at times, as the compensation would be insufficient to pay for supervision.

This objection to the plan would, however, be removed, if it can be demonstrated that the better class of persons will receive and care for such patients; but when we con-

sider how extremely reluctant the larger number of these persons are to have even their own relatives, so affected, at home; how unwilling they are to be burdened and hampered with the responsibility of their care; and, indeed, how ready they are to make great effort, and sacrifice much, that they may be able to pay others for this duty rather than do it themselves, we may surmise that it will not be easy to find those who, with the requisite qualifications, and without any tie of blood, would be willing to undertake the charge.

It is not, however, intended to assert that persons cannot be found who will be more than willing to undertake this kind of occupation for the remuneration it would furnish, but it would be safe to assert that they would be such as would be without responsibility or qualifications for the charge. But lest I may be considered as regarding this matter with a bias, I will refer to the views of a commission which, three years since, was appointed to examine into the condition of the insane poor of Connecticut. Referring to this matter of boarding of patients, the commission uses the following language: "At Tarriffville we found about ten who were insane or imbecile. They ought not to be there; it is no proper place for these feeble persons, and it is an outrage upon humanity, a disgrace to the civilization of the state, and a sad reflection upon our Christianity, that they are there at all. We will not be silent, or smooth this matter with pleasant words, and end with saying, 'this is all very well for *them*.' It is not well for them, and no one with the love of God in his heart and true love for his fellow-men can honestly say it. We shed our tears over the distant and repulsive Fejii islander, and mourn the wrongs of the barbarous Sioux Indian, but here, right here, in this Christian commonwealth, are men and women kept day after day, by the

authority of the state, in a condition which should shame a savage, or tingle the cheek of an infidel." "And it should not be forgotten that brutal men and brutal women, and sometimes those, too, who would shudder if such terms were applied to them, are guilty of the most intolerable cruelty to the insane, in their management of them, or in their efforts to carry out a fanatical idea that these persons 'must be made to mind.' Such cruelties, if committed in an insane asylum, would arouse the land."

The commission found this a specimen, it is to be hoped an uncommon one, of the care some of the unfortunate insane poor of Connecticut experience, when exposed to the boarding-out system. How many such cases would be found, indeed, how many now exist, may be inferred from the experience of the past, and our knowledge of the tendencies of human nature when influenced by selfish interests.

We will now refer to the second plan mentioned above, viz.: Placing them in homes or asylums specially constructed for and adapted to the care and use of such persons.

1. This plan is free from the principal objection urged against the first plan—a lack of *inspection*. This provides for daily, and, if need be, hourly inspection by persons educated and trained for the purpose. The amount of labor to be required of any one would be decided by the physician in charge.

Peculiarities and physical conditions would be considered, while temporary illnesses could be treated at the time and without removal. A considerable number, one or three hundred, could be cared for by a very few persons, and these persons would be constantly under the guidance of one who, with themselves, will be entirely free from bias or interested motives; consequently, the amount of labor

to be required will depend simply on the well-being and happiness of the patient, and not on the personal need or advantage of another party. They could be easily classified and arranged in companies according to peculiarities and conditions existing, and this consideration is one of much importance in reference to labor. The influence of example is very potent with this class of persons, so that the few in any company who might not be disposed to join in the labor would soon be led to do so by the others, thus increasing the amount secured.

2. They could be easily and frequently visited by those duly authorized by the state for that purpose, and their general condition and care determined.

Such homes or annexes could be built near to the state asylums, so that persons becoming excited or depressed can readily be transferred to them during these periods, and receive the necessary care with little inconvenience.

But, it is objected, that by such a method of caring for large numbers of the chronic insane, they will be deprived of their liberty in a larger degree than is necessary. Now, as this is a serious matter, and may apply with equal force to the care of all classes of the insane when considerable numbers are in one home, it may be well to consider it somewhat carefully.

There is magic in the very sound of the word liberty to every English-speaking person, and we are ready to exclaim with Sterne: "'Tis thou, thrice sweet and gracious goddess, addressing myself to *Liberty*, whom all in public or in private worship, whose taste is grateful and ever will be so, till Nature herself shall change."

But, there is a vast difference between liberty for the sane and insane. To the one it means freedom to care for himself and family; to labor daily for their support and comfort; freedom to do the most he can as a citizen for

his neighborhood and his country; freedom to acquire property and make provision, as best he may, for the dark days of sickness and old age; freedom to participate in the election of those who may exercise authority in state and national councils; freedom in the pursuit of happiness for self and family; freedom in the worship of God. “No *tint* of words can spot thy snowy mantle, or chymic power turn thy sceptre into iron; with thee to smile upon him as he eats his crust, the swain is happier than his monarch from whose court thou art exiled.”

But liberty for the demented, the excited, the depressed, the deluded, the weak in body and mind, the odd and singular in general conduct, signifies something vastly different from this—indeed, quite the opposite. Liberty in this case means the wandering about the country, according to one’s own sweet will; the sleeping in barns or under haystacks in the fields; the dressing in grotesque and fantastic styles; the being hooted and jeered at by thoughtless boys; working or not as one may choose; the being sought after at night; and exposure to wet and cold. It signifies an aimless and objectless kind of life, the doing as one may please without much reference to the consequences, the going hither and thither, a restless and uneasy desire and effort for one knows not what. Unrestricted liberty means this, if it means anything for the insane. Few insane persons of the class under consideration are anxious for the kind of liberty sane persons desire to enjoy and glory in, but simply the liberty of license, and it seems to me important to recognize this distinction in our desire to do all that may actually be for their highest good and happiness.

The truth is, and we all must recognize it, that these persons must have supervision in one form or another. All supervision implies restriction, and all restriction means deprivation of the liberty desired.

Disease, in all forms, restricts personal liberty, and often is a tyrant, especially with the insane. Therefore, in our care for them it should be one of our aims to make the burden as light as possible, to grant all reasonable requests, to give all reasonable liberty with necessary restrictions.

The exercise of supervision and consequent restriction of license implies the largest kindness on the part of those having the care of them; and when gathered in considerable numbers, a larger degree of liberty in reference to personal movements, labor and amusements, would be practicable than when isolated in farm-houses, and under the care of one person, and he an interested one.

But, again, it is claimed that life, in any kind of asylums and with considerable numbers, is *routine, monotonous, and unhomelike*.

Well, the same may be said to be true as to life in boarding-houses and hotels. There is the same general round of daily meals, the gathering in the reading-rooms, the smoking-rooms and vestibules, the same routine of work, and not unfrequently hard work. In fact, life is routine for the vast majority of people, especially the poor, everywhere.

Still, people, many of them, prefer to live in boarding-houses and hotels rather than in private homes. The truth is, however, that life in such an asylum would be vastly more varied and attended with much more in the way of amusement, interest and diversion, than is possible with the vast majority of people who, in the same station of life, live and labor in their own homes; and certainly vastly more than would be the case if they were farmed out singly in the homes of country laborers, and required to conform to their habits and modes of living. It would be a mistake to suppose that because some of these persons are restless, uneasy, and unhappy, they would be less so under other circumstances. They would be unhappy anywhere and

under any regulations of life, because they are not in health.

It may be proper here to add a word in reference to the arrangement and construction of such asylums as these now under consideration.

Buildings for the care of such persons could be located in the vicinity of state institutions and on a large plat of ground especially adapted for farming purposes. They should be provided with shops and work-houses for the occupation of such patients as have been accustomed to mechanical pursuits.

The buildings may be under one roof, and arranged with day and night rooms, with amusement- and smoking-rooms, and rooms for games; or they could be in detached houses connected to the central and administrative building by means of covered corridors. One or two farm-houses, sufficient for families of eight or ten persons, could be located on the border of the grounds, and be in charge of a farmer and his wife.

The larger portion of all these buildings could be without locked doors and guarded windows, a limited portion being provided with them for such as could not be trusted.

All persons in charge of these patients, from superintendent to attendants, should be selected with special reference to their duties and the method to be employed in their discharge. They must be thoroughly in sympathy and identified with the plan to be pursued. They must be with, live with watch over and direct the movements and labors of these patients, as well as their amusements, as *friends* rather than masters. They must consider them as wards or children rather than responsible persons.

It is believed that life under such a plan and conditions would be no more irksome or routine than life in hotels or in one's own home; that it could be made remunerative to

a considerable extent; that it could be freed from many of the restraints which appear to be necessary for the acute and some of the chronic insane.

In reference to the third plan, viz., that now in use in our asylums, it may be said that it provides for oversight and frequent inspection; it provides for the care of the many by the few, and it provides for their economic care by those who are above personal bias. In all these respects it would appear to be better than the first plan examined.

It is, however, thought by some to be objectionable in its general effects.

1st. It tends to crowd our asylum capacities, and thus keep many of the more acute and curable cases from receiving treatment in them.

2d. This class of patients occupy room in asylums built especially for the care and treatment of the curable class, and, therefore, at a considerable larger outlay than would be deemed necessary for the chronic incurable class.

3d. By being associated with the acute and curable class, and at the same time deemed and treated as incurables, the class of chronic insane under consideration *may* become in time somewhat neglected.

The capacities which they have left, and which, under a special line of care and treatment, might be developed so as to render them still larger, *may* be overlooked by those who have a so much larger duty and interest in caring for the curable class. It is thought that by being in asylums built especially for them they could be more largely occupied in labor, be entrusted with a larger amount of freedom as to personal exercise, and more likely to become partly self-sustaining, thus ensuring a larger degree of happiness to themselves and a less expense to the state.

There may be some difference of opinion as to how important the above considerations may be, but I am inclined

to the opinion that they are well taken. I think buildings and equipments for the kind, judicious and economic care of the class of persons now under consideration, can be built at a considerable less expense than may be requisite or desirable for the curable class; and when we take into consideration the large number of these persons which is every year added to our asylum population, and how large that number must become in the no distant future; when we consider the millions of money already expended in some of the states, and the additional ones soon to be called for if the present style and cost are to be continued, the importance of this consideration becomes more apparent.

I am inclined to the view that a larger amount of happiness and productive industry can be obtained, a greater degree of liberty in personal movements may be had, when in homes built and conducted with special reference to their peculiarities and requirements; where they are under the care of physicians and attendants who are free from other and more urgent calls in connection with another class of the insane, and, consequently, can look exclusively to their more immediate interests, management, and occupation.

In the treatment of the subject as above, it is not claimed that there may not be individual cases of the insane who could be properly cared for in private homes. It is not claimed that there may not be persons who could and would take care of these individual cases.

But the subject has been considered in relation to such numbers as would be of importance in relation to the state, and thus become a matter of public interest.

INFECTIOUS (SO-CALLED ULCERATIVE) ENDOCARDITIS.

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UNDER the terms *diphtheritic*, *ulcerative*, *malignant*, *septic*, or *infectious* endocarditis, *arterial pyæmia*, *mycosis endocardii*, physicians now recognize one of the most formidable of cardiac affections, characterized by a peculiar morbid process on the valves, blood contaminations, constitutional symptoms of the typhoid or pyæmic types, and usually associated with multiple emboli.

It is only within the past few years that the subject has received due attention in the text-books ; indeed, in some it is barely touched upon, and even in recent manuals on heart disease the account is not very satisfactory.

From the number of reported cases in French and German journals, and from the interest which the disease has excited in these countries, we might suppose it to be more common there than in England or America. A considerable number of reports, however, occur in the "Transactions of the Pathological Society of London" and in the British journals. In the leading American periodicals there are very few references, but cases have

been reported by Ellis,* Lomax,† Pepper,‡ Keating,§ and Peabody.||

With regard to the nomenclature, I think the terms *infectious* and *septic*, as given by Jaccoud,¶ better than the others. Against the name *ulcerative* is the fact that there may be no actual ulceration on the valves, and there may be, on the other hand, endocardial losses of substance without the special constitutional disturbances by which the disease is characterized. The term *diphtheritic* is good, in so far as it expresses a resemblance in the histological features of the valvular disease to that of true diphtheritic exudation, but this is scarcely sufficient ground for its use; and it is, in a way, misleading, indicating a relation between diphtheria and the disease, which is not known to exist. The name *mycosis endocardii* certainly expresses a striking feature of the local process, but with our present imperfect knowledge of the relation of the micrococci colonies to the disease, such a designation is, to say the least, premature. On the other hand, the term *infectious* presupposes no special view as to the nature of the local process, and at the same time indicates, as Jaccoud says, a constant and exclusive character of the disease.

It would appear that, clinically, three classes of cases are included in the disease known as ulcerative endocarditis, and I think it important that a distinction should be made between them. We have:

1. Those cases in which the disease appears without any obvious cause, either spontaneously or in connection with rheumatism or some other affection. The term *infectious*

* *Boston Med. and Surg. Journal*, Nov. 15, 1877.

† *Philadelphia Medical and Surgical Reporter*, 1874.

‡ *American Journal of Medical Sciences*, 1871.

§ "Transactions of the College of Physicians of Philadelphia," 1879.

|| *New York Med. Record*, 1880.

¶ *Pathologie Interne*, tome i, and *Nouveau Dictionnaire*, tome iii.

might be applied to this class. It is the *arterial pyæmia* of Wilks, the primary ulcerative endocarditis of some authors.

2. Those in which the endocardial disease is secondary to some inflammatory focus—acute necrosis, puerperal endometritis, etc. To these the term *septic* might be applied.

3. In certain cases of chronic valvular disease an acute endocardial process may be engrafted (recurrent endocarditis), presenting anatomical features similar to the infectious form, but not characterized by the same clinical picture, the patients dying with the symptoms of chronic heart disease.

The following paper embodies my experience of this disease. The chief points to which I wish to call attention, and which are illustrated by the cases, are :

1. That the majority of cases of infectious endocarditis occur independently of rheumatism.

2. To the frequency with which infectious endocarditis is associated with pneumonia.

3. The production of acute multiple aneurisms of the aorta in the disease.

4. To certain histological features in the endocardial vegetations, and particularly to a remarkable fungoid growth met with in one of the cases.

CASE I.—In January, 1878, I received from Mr. McEachran, of the Montreal Veterinary College, the heart and part of the aorta of a cow which had died with symptoms of urgent dyspnoea. He saw it only a short time before death, and no satisfactory history of the case could be obtained. The weather was very cold, and the heart was frozen immediately after its removal, and in this state I received it.

The organ was large, particularly the ventricular portion ; the chambers had been cut open, and the blood had escaped. On exposing the tricuspid orifice, from the auricle it appeared to be almost closed with irregular vegetations attached to the valves. From

the ventricle the following condition was presented : The segment next the septum was completely covered on its under surface with a grayish-white outgrowth, which was prolonged at the apex and extended about half way over the auricular surface. The chordæ tendineæ were entirely covered, and similar masses extended down the septum, forming irregular warty projections, some of which were the size of large cherries. The other segments were not so much involved, but in both the growth was most extensive on the ventricular surface, and irregular masses projected from the tips of the cusps, which resembled somewhat the comb of a cock. The chordæ tendineæ were uninvolved. Pulmonary semilunar valves healthy ; mitral valves unaffected. Aortic orifice blocked with vegetations similar to those in the tricuspid. On slitting up the vessel the segments of the valve were found much crumpled and covered on the ventricular surfaces with warty outgrowths, some of which were over a centimetre in length.

In the ascending portion of the arch there were several small outgrowths on the endocardium, and near the terminal portion of the arch there was a much larger, irregular mass. All of these structures presented a similar appearance—grayish-white in color, of moderate consistence, but on firm pressure somewhat friable. They were very closely adherent to the parts from which they grew. An outer cortical and an inner parenchymatous part could be distinguished. It appeared a typical example of a verrucose endocarditis. The other organs were examined, but I could get no information as to the presence of infarcts.

CASE 2.—*Infectious endocarditis ; pneumonia ; meningitis.*

Mary D., aged 29, admitted to hospital October 22, 1878, in an unconscious state. She is a married woman, and has two children. Has been a hard drinker for several years. History of the onset of the attack could not be satisfactorily obtained. On the 23d, when examined, she was still unconscious ; pupils moderately dilated ; no twitchings or paralysis ; slight dulness at right apex, with blowing breathing and râles ; systolic murmur at apex. T. 104° ; P. 110 ; R. 40.

On the 24th, she was partially conscious for a short time, and complained of great pain in the head and back of the neck. Morning, T. 100° ; Evening, 104°.

Throughout the 25th she lay in an unconscious state ; passed fæces and urine in bed. Evening, T. 104° .

On 26th, temperature rose to 107° at 4 A.M. ; was 105° at 2 P.M. There was a slight divergent strabismus of left eye, and commencing superficial ulceration of left cornea. Right pupil dilated widely. Death took place at 4 P.M.

Autopsy.—In the *heart*, ventricular surface of anterior segment of mitral valve was covered with grayish vegetations ; toward the right side of the valve they were larger, and extended to the chordæ tendineæ, passing down the entire length of several of them. On the auricular surface of the valve there was a soft, grayish-white patch, 1 by 1.2 cm., covered in part by a thin membrane, but in the rest of its extent rough and divided into a number of irregular projections, which were friable and readily detached. The other valves were healthy. In the right *lung* the upper lobe was in a state of red hepatization ; toward the anterior border the process was more advanced, and a sero-purulent fluid bathed the surface. The upper third of the lower lobe was also inflamed. In the *brain*, meninges at the base were matted and œdematous, but there was no exudation. On the hemispheres there were numerous patches of lymph beneath the arachnoid, situated chiefly in the anterior regions. The posterior margin of corpus callosum and contiguous surface of cerebellum were covered with a thick, creamy exudation.

Spleen presented a single infarct. Organ a good deal enlarged.

Kidneys healthy. Nothing special in other organs.

CASE 3.—*Infectious endocarditis ; pneumonia.*

J. B., aged 38, admitted January 7, 1880. Has been a healthy man.

Ten years ago had a severe attack of pneumonia. On the night of the 4th he felt uneasy, and did not rest well ; got feverish, and in the morning had pain in the side, with cough. These symptoms continued, and he came to the hospital on the 7th. On admission, T. 103.8° ; P. 128 ; R. 40. Signs of pneumonic consolidation in right lung ; dulness from second rib in front, and extending into the lower axillary region and the base posteriorly. There were blowing breathing, râles, and increased tactile fremitus. The expectoration was viscid and rusty. During the first week in hospital nervous symptoms appeared ; he became delirious, rest-

less at night, and passed urine and fæces in bed. Tongue dry, and on the 9th and 10th there was troublesome vomiting. The temperature was irregular, ranging from 100° to 104° ; the evening record was usually a little higher, but twice it was lower than in the morning. Pulse range 120 to 148; respiration 32 to 50. During the second week the intensity of the symptoms abated somewhat; the temperature kept lower, not once reaching 101° . Respiration diminished in frequency, and the pulse range was from 112 to 120. The nervous prostration continued, with tremor of the whole body; the muscles of the face and hands twitched constantly. Delirium persisted, and discharges were passed involuntarily. A very disgusting fetor emanated from his body. The cough improved, and the dulness diminished somewhat in front. Tongue dry; took food and stimulants freely.

On the 19th, a painful swelling appeared in left parotid region. In the third week he began to have chills, and sweated a great deal each day. The swelling in left parotid diminished, and the lung cleared. The prostration continued and the delirium persisted, but the twitching moderated. The temperature was very irregular, usually below 100° , but on two occasions it went up to 103° after chills. Pulse range from 116 to 130.

During the fourth week the swelling of left parotid increased, and on February 1st an abscess was opened in this region. Severe chills on the 30th, blueness of face and finger-tips. T. 102° . Still sweats. Became somewhat brighter after the abscess was opened. Tongue dry; nervous symptoms less marked. No cough. Pulse feeble, range 108 to 120. Temperature 98° to 100° ; on three days after chills it rose about 102° .

In the fifth week he remained in this typhoid condition, with very little change; an occasional chill and profuse sweats.

During the sixth week the prostration increased, and he lay in a heavy, unconscious state. Tongue dry and cracked; no chills, but profuse sweats.

On the 13th and 14th, T. began to rise, and reached 104° . Muscular tremors again set in, and death took place on the 15th, after an illness of 42 days.

Autopsy.—Body wasted; in preliminary incision thoracic and abdominal muscles pale.

Heart of average size; not apparently hypertrophied. Coagula in all the chambers. Valves on right side normal. In left ventricle a large mass filled the outer angle of the mitral orifice, looking like a fibrinous clot between the valves, but on closer inspec-

tion it proved to be a large endocardial vegetation. Viewed from the ventricle, the outer half of the aortic or anterior segment was involved, and the disease had penetrated the entire thickness of the valve, projecting in grayish-white, flattened masses between the points of attachment of the chordæ tendineæ. On this surface it extended to within 1.5 cm. of the semilunar valve. The posterior mitral segment was not so much involved on this surface, but at the outer angle between the two flaps, the mass was very thick, and extensions from it passed along the chordæ tendineæ to the top of the posterior papillary muscle. The full extent of the disease was seen when the mitral ring was laid open—a thick grayish mass encrusted the auricular surfaces of the outer halves of both segments, filled the angle between them, and extended up the wall of the auricle. On this aspect it measured 3 by 2 cm. The surface of the mass was nodular, in great part of its extent unbroken, and covered with a thin membrane, which could be lifted up. In places there was extravasated blood beneath this thin coating. The portions upon the wall of the auricle and on the contiguous part of the valves were roughened and granular. The anterior curtain was most affected, but the vegetations on the posterior projected much more. Section through the mass on this segment gave a thickness of 12 mm.; no proper tissue of the valve could be seen, but only a uniform, finely-granular, grayish-white tissue. Aortic semilunar valves healthy. Aorta not atheromatous.

Lungs crepitant in upper and anterior parts, heavy and œdematous posteriorly; the tissue of the right lung at the base was firmer than that of the left, but the section was not granular.

Spleen large, weighed nearly 400 grammes; pulp very soft; one wedge-shaped infarct of grayish-yellow color.

Kidneys pale; no infarcts.

Liver soft, and of a muddy-brown color. There was nothing of special note in the *stomach* or *intestines*. Peyer's patches not swollen.

The *brain* presented nothing abnormal.

In the *left parotid* the abscess had nearly healed.

CASE 4.—*Infectious endocarditis; pneumonia; meningitis.*

M. W., aged 43, a tall, well-built man, was admitted to hospital under Dr. Ross, Feb. 26, 1880. Served his time (21 years) in the British Army. Has had syphilis, and only a month ago was under treatment in ward 11, for syphilitic ulcers in right

gluteal region. On Oct. 27, 1879, he was admitted with pneumonia of lower three-fourths of right lung and had severe cerebral symptoms. He has been a very hard drinker.

On evening of Feb. 23d had a severe rigor followed by fever, headache, cough, and pain in the left side. On admission T. 100° , R. 38, P. 120. Cough with viscid expectoration. Has spells of vomiting and feels very weak. Examination of chest revealed dulness, blowing breathing, and crepitant râles at left base as high as angle of scapula.

27th. T. M. 101° , E. 101.4° , P. 128, R. 34. A friction sound is heard just above the angle of the scapula on the left side.

28th. T. M. 99.4° , E. 100° , R. 36, P. 114. Patient became delirious through the night, expectoration profuse and blood-tinged.

March 3d. For the past three days patient has been improving slowly; cough not so troublesome; no special change in the physical signs. Temperature has fallen and has been only 98° for the past three mornings. Is free from delirium.

4th. Patient had a chill at one o'clock P. M. accompanied by vomiting, and the temperature rose to 101° .

5th. Had a restless night, delirious again, no extension of the disease in the lung; at two o'clock P. M. had a chill, and the temperature went up to 103.5° , P. 104, R. 40. Has had five stools.

6th. Morning T. 98° . Patient is very prostrate, passed a restless night, there is a low wandering delirium.

7th. T. rose to 103.5° from 98° during the morning; very profuse diarrhœa, 10 stools.

8th. Morning T. 100° , E. 104.3° . Dulness persists at left base, râles more liquid in character; diarrhœa is better.

9th. Patient is in a low typhoid state, tongue coated and dry. T. went up to 105.3° in the evening, R. 36, P. 126, and feeble.

10th. Profuse diarrhœa, nine stools; is very prostrate. P. 124, R. 36, T. morning 101° , evening 102° .

From this time until his death on the 14th he gradually sank, remaining unconscious. The temperature range was from 101° to 104° , the evening exacerbation being usually about three degrees. On the 12th there were signs of œdema at right base. The amount of urine passed ranged from 40 to 50 oz., acid in reaction; there was albumen on the first three days after admission. Chlorides were diminished; on the 2d of March they were absent.

Autopsy. Nothing special on superficial inspection. *Brain*; at base membranes a little œdematous, no lymph. An aneurism the size of a pea projected from the central part of the basilar artery and has formed a bed for itself in the pons. There was considerable meningeal inflammation on the left hemisphere; the posterior part of the 1st frontal and the ascending frontal convolutions were covered with a thick creamy lymph, and a similar condition existed along the outer part of the Sylvian fissure and over part of the ascending parietal convolution and the superior parietal lobule. On the right side there were a few patches of lymph along certain of the vessels, but none on the convolutions. Nothing special noticed in the substance of the organ. *Heart.* All the chambers contained blood and partially decolorized clots. Left ventricle looked a little large. Mitral orifice not enlarged, valves thin and healthy-looking; on auricular surface of the edges numerous small bead-like vegetations. Aortic orifice obstructed with large irregular vegetations, which, on slitting up the vessel, were seen to spring from the right posterior segment. The ventricular surface of this valve was almost covered with a grayish-yellow outgrowth irregularly divided into two portions, the pointed ends of which were covered with closely adherent blood-clot. The surface of these masses was smooth, though nodular; it was only in the cleft between them that the granular substance of the vegetation was exposed. From the arterial side it was seen that one-half of the valve was completely destroyed and the gray-green tint of the substance of the growth was here very marked. There was one small patch on the ventricular face of the anterior segment. *Lungs.* Lower three-fourths of the left lung heavy, airless, reddish in color, except at anterior border where it is more anæmic. Granular condition of section not very distinct. Pleura of this part covered with a thin exudation. Other parts of the lungs crepitant. *Spleen* is large and very soft; no infarctions. *Kidneys.* Left presented one small infarct the size of a pea. Organs are soft and cortices a little swollen. *Liver* pale and soft. *Intestines* pale, no ulceration.

Cases 3 and 4 offer typical examples of this disease;—the chills, irregular temperature, sweats, and diarrhœa were very characteristic. Case 3. conformed more to the typhoid form, Case 4 to the pyæmic variety.

CASE 5.—*Chronic valvular endocarditis ; recent endocarditis and endarteriitis ; multiple aneurisms of aorta ; rupture into pericardium.*

Robert I., aged 29, a hospital orderly. When seven years old had a severe burn in right axilla and front of chest, which has left a large scar. Has been troubled with palpitation since a lad, and during the past few years this has become worse, particularly on exertion. Has been a sailor. In 1876 was treated in the hospital for syphilis, and was told he had aneurism. He had a sharp attack of quinsy in February, 1879, and in the clinical report it was then noted "that the heart was somewhat hypertrophied, double murmur at base, and a distinct systolic thrill could be felt in the aortic area." Unfortunately the notes of his final illness are very scanty. I have been furnished with the following by Dr. Imrie, the House Surgeon : Patient was readmitted on June 4, 1880, with a history of diarrhœa of several days' standing, chills, headache, dyspnœa, cough and fever. On examining the lungs there were signs of pneumonic consolidation at left base, dulness, blowing breathing, râles and exaggerated fremitus ; temperature 104° , and he became delirious the same evening. Heart embarrassed ; distinct double aortic murmur, and basic thrill. The inflammation of the lung extended and involved nearly the entire organ. There was great nervous prostration, a low delirium, and distinct chills at intervals. The temperature ranged from 99° to 105° ; death took place on July 1st.

Autopsy.—Body somewhat emaciated. In thorax there was a rounded tumor beneath the first piece of the sternum, and which passed to the right beneath the first two ribs and the clavicle. It was quite soft and had no superficial adhesions. Pericardial sac looked large, and when opened, 18 ounces of blood and clots were removed. The source of the hemorrhage was discovered to be a laceration in an aneurismal pouch which projected into the pericardium from the ascending aorta.

Heart.—Auricles contained blood and thick clots ; there were numerous small ecchymoses beneath endocardium of the right side. Right ventricle small in comparison with the left ; tricuspid and pulmonary semilunar valves healthy. Left ventricle dilated and hypertrophied, the walls unusually thick. Mitral orifice measured 11 cm. in circumference ; valves opaque ; chordæ tendineæ thick ; aortic valves incompetent ; segments thick and curled at the edges ; the anterior and left posterior segments have

fused together, and from the ventricle, presented the appearance of a single curtain, but on the arterial side, a median raphé passed half way up the segment and divided the sinuses incompletely. Attached to the thickened border were four grayish vegetations, the size of small peas; and on the right posterior segment, a large flat one covered nearly one half of the ventricular face of the valve. On the endocardium of the ventricle, just below the aortic ring, there was an elevated flattened mass the size of a five-cent piece. Immediately above the right posterior segment, two large grayish-yellow vegetations projected from a slight depression in the wall of the aorta and were in contact with the edge of the valve. A sort of cleft separated the two masses, and when probed, was found to lead into a saccular pouch the size of a large marble, from the edges of which the outgrowths arose. The walls of the small aneurism were thin, composed chiefly of the adventitia, and had lost the appearance of an arterial coat. The interior of the arch was smooth, with the exception of two small patches of superficial atheroma. Two and a half centimetres above the valves the arch measured 8.5 cm. in circumference. At the junction of the ascending and transverse portions, about 1.5 cm. from orifice of innominate, there was a circular opening the size of a fifty-cent piece, leading into a saccular aneurism, the size of a small apple, which projected to the right side and was partially enclosed in the pericardium. The edges of the opening presented large fungoid vegetations, attached to the margin of the intima, and projecting in some places as much as 1 cm. The sac contained blood and recent clots, but no laminated fibrin; the walls were exceedingly thin, in places quite translucent. The vegetations at the edge of the orifice extended upon the inner surface of the sac, covering it in at least half its extent, and in places infiltrated the entire thickness of the wall, so that the peculiar greenish-yellow color of the growth could be observed from the outside. In the wall of the aneurism, just within the line of attachment of the pericardium, there was an irregular laceration 1.3 cm. in length. On the intima of the ascending portion, just below the margin of the aneurism, were two small warty outgrowths which, when carefully examined, were found to spring from the edges of small lacerations or losses of substance, behind which were two aneurismal pouches, about the size of large peas, the walls thin and formed chiefly of the adventitia, which appeared dissected away for a short distance around the narrow break in the internal and middle coats. In one the vegetation extended

round the edge of the orifice upon the outer surface of middle coat, and into the angle between it and the adventitia.

Lungs crepitant in anterior parts; bases heavy and sodden; on section much blood and serum escaped. The left lower lobe was firm, almost airless, but had not a granular appearance on the cut surface.

Spleen weighed 560 grammes; pulp very soft; two small yellowish-white infarcts, tolerably firm and surrounded by deeply congested tissue.

Kidneys not enlarged; the right organ contained one small yellowish infarct.

Stomach and *Intestines* presented nothing of note; no ulceration in small bowel, or enlargement of Peyer's glands.

Brain. Meningeal hemorrhage beneath arachnoid, chiefly on the right side, upon the sphenoidal convolutions and along the fissure of Rolando; it was thick enough to obliterate the outlines of the convolutions. On the left side there was a thin extravasation over the second and third frontal and upper half of the ascending parietal convolutions. No lesions of the vessels were found; substance healthy..

CASE 6.—*Fractured legs; pneumonia; ulcerative endocarditis; meningitis.*

Unfortunately, the notes of this case have been mislaid, and I am only able to give a brief summary. The patient, a young woman aged 19, jumped from the third story window of a hotel, during a fire, and sustained a double Pott's fracture, and fracture of lumbar vertebræ. She seemed to be doing very well for about a week, when the temperature rose and she complained of pain about the heart and shortness of breath. A systolic murmur in the mitral area was detected. She became exceedingly weak and prostrated, blood appeared in the urine, and she died sixteen days after admission.

At the autopsy, there was no suppuration about the fracture. The *lungs* were engorged with blood posteriorly, and the lower part of right upper lobe and contiguous parts of middle and lower lobes were hepatized. The *heart* was not enlarged; on the anterior curtain of the mitral valve was a large endocardial outgrowth, involving the anterior part of the valve and extending on to some of the chordæ tendineæ; a blood clot was adherent to it, but its surface did not seem broken. A small infarct in *spleen*, and two

in *kidneys*. There was purulent infiltration in the sulci, beneath the arachnoid on the cortex of the *brain*, chiefly on the parietal and frontal lobes; none at the base.

In the *cæcum* and ascending *colon*, were eight or ten superficial patches of membranous colitis, the exudation grayish-yellow, thin, and situated upon injected mucosa.

This case comes, doubtless, under the second class, in which the endocarditis appears to follow some injury or wound. In going over the literature of the subject, I noticed the reports of a considerable number of cases of this kind. Dr. Peabody's case, the autopsy of which I witnessed, resembled this in all its essential features. I should like to remark here that the endocarditis in many cases of this class is a secondary and subsidiary phenomenon in septic infection. Thus, in acute necrosis and in puerperal septicæmia, it is sometimes present, sometimes absent, and the fatal effect and malignancy of the affection does not appear dependent upon it.

CASE 7.—*Sclerotic endocarditis of aortic valves, with incompetency; recent vegetations (ulcerative endocarditis).*

Annie M. L., aged 40, admitted Nov. 23d under Dr. Ross.

No history of acute rheumatism. For five or six years has suffered with shortness of breath on exertion. For the past year health has been failing, and she has had a troublesome cough. For three months has been confined to bed; dropsy has gradually come on, and for three weeks past spitting of blood. Her condition on admission was that of a patient in the advanced stage of obstructive heart disease,—great dropsy of legs, right hydrothorax, dyspnœa, lividity, cough, and bloody expectoration. A double aortic murmur was determined. She only lived for a little over two days after entering the hospital. The temperature was normal.

At autopsy *heart* was large, chambers full of dark clots. Mitral valves healthy. Left ventricle dilated and hypertrophied. The aortic orifice was blocked with vegetations, and when slit open the valves were found much diseased; all the curtains were thickened, curled at the edges and foreshortened. On the ventricular faces were large grayish-yellow vegetations, closely adherent, but friable and roughened on the surface. In one mass the deposition of salts of lime had taken place on the outer part. Large patches of apoplexy in the *lungs*. No infarcts in *spleen* or *kidneys*, which were large and indurated.

This is an illustration of the third class, and perhaps such instances furnish the large proportion of the cases which go under the heading of ulcerative endocarditis.

General and microscopical characters of the vegetations :

With the exception of the specimens from Case 1, the outgrowths on the valves presented the well-known appearance of the so-called diphtheritic endocarditis. There are one or two points in connection with their general character to which I wish to refer. The term *ulcerative*, as I remarked before, is in certain instances a misnomer. The vegetations on the valves in Case 6 presented a smooth surface, neither granular nor broken, and there were no signs of separation at the attached border. One or two writers have remarked upon this, especially Gray, of Oxford.* Usually, however, the surface of the vegetations is roughened in places, and the friable stroma exposed; and of course if the entire mass were removed there would be an ulceration or even perforation of the valve. We do not know much about the beginning of the process, but it may be that the ulceration comes first, and the thick vegetations represent subsequent formations in the exposed surface.

About the vegetations in Cases 2, 3, and 4, there was a peculiar greenish-gray color, especially marked when they were broken. It was common to meet with a blood clot adherent to the masses, and frequently a thin superficial extravasation beneath the outermost layer of the vegetation.

In Case 1 the vegetations were firmer, not so friable, and had rather the characters described as verrucose.

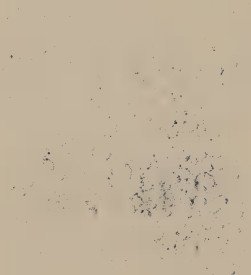
In Case 5 the outgrowths on the aorta and at the margin of the larger aneurism were soft, of a light grayish-yellow color, and the term "fungoid" best expresses their general appearance. The valvular outgrowths in Case 7 presented

* *Med. Times and Gazette*, 1874.

the same general characters as in the other specimens, except in the slight calcification at one part.

The microscopical characters of the vegetations in Case 1 offer many interesting features. In the study of this specimen we will begin with the description of small outgrowths. Fig. 1 represents the section of a small wart-like excrescence on the wall of the right ventricle. It is mushroom-shaped, measures 3 mm. across, and springs from the endocardium by a small pedicle. There is no special change in the heart muscle immediately below it (*a*). At the site of attachment the subendothelial tissue is thickened, and contains in the deeper part many nucleated corpuscles imbedded in an indifferent matrix, while in the more superficial part it is distinctly fibrillated, and large elongated corpuscles occur. The vegetation is attached directly upon the fibrillar layer, with the intervention of a thin stratum of round and elongated cells. At *b* in the figure there appears to be an additional base or stalk, and here the proliferation of the subendothelial elements was very marked. The pedicle itself is composed of closely aggregated corpuscles of the size and general appearance of white blood corpuscles. The material in which they are imbedded is granular; fibrils cannot be detected. An irregular break, probably the result of manipulation, occurs about halfway across the mass. At the edges of this (*c*) the colorless cells are thickly set and are stained deeply. The stroma of the mass is made up of a dense fibrin meshwork, only seen with a high power and in a thin section. It is variously interspersed with cells; from some places they are entirely absent. Toward the surface the fibrin assumes a stratified disposition, and the corpuscles are less numerous (Fig. 2).

A short distance from the pedicle, ball-like masses are seen imbedded in the fibrinous stroma, and at the superficial part of the mass similar bodies are very numerous and



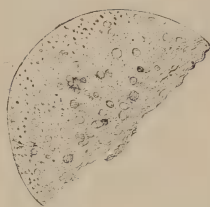


Fig. 2



Fig. 1

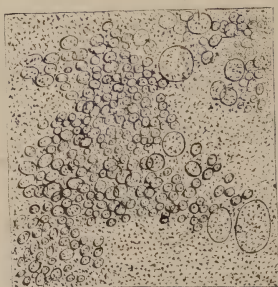


Fig. 4

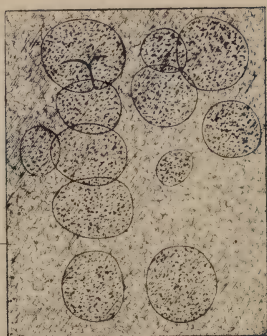


Fig. 5

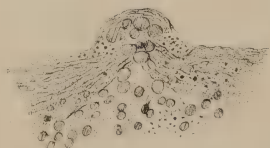


Fig. 3



Fig. 6

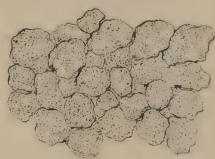


Fig. 7

constitute the most remarkable feature in the entire texture, Fig. 1, *d*; Fig. 2.

Many of the tendinous chords passing from the tricuspid curtains were thickly encrusted, and sections afforded a good view of the general arrangement of the parts. In a section of such an encrusted tendon, 6 mm. across, there can be seen the tendon in the centre, 1.5 mm. in diameter. Under the microscope it does not appear much altered, and it is only at the periphery that there is any nuclear increase; outside of this is a layer devoid of cell elements, finely granular, and in places laminated. In logwood this part does not stain so deeply. In it are remarkable micrococcus balls, some of large size and isolated, others smaller and closely aggregated together (Figs. 4 and 5). External to this layer and separated from it by a small amount of granular matter is a narrow zone of fibrinous tissue, in which elongated corpuscles are very abundant. It looks as if this was the outer part of the tendinous chord, and as if the layer just described had developed beneath the subendocardial tissue. The external part, comprising the greater portion of the section, is made up of a fibrinous matrix, containing leucocytes scattered through it; most abundant in certain areas. The outermost part of the encrusting mass is distinctly laminated, and contains very few corpuscles, but is thickly set with micrococcus balls, and the tissue has a darkly granular appearance. Fig. 3 shows the stratified appearance and the spherical bodies which here form a nipple-like projection, and appear as if passing toward the surface. This appearance is by no means infrequent.

In the larger outgrowths the chief mass is composed of a nucleated fibrillar tissue, while in the superficial parts there are fibrinous lamination and numerous micrococci colonies. Capillary blood-vessels penetrate the deeper parts of the large masses, and along many there is a deposition of

brown-red pigment. In some sections large micrococcus balls were met with 4 or 5 mm. from the surface.

The most remarkable structures in this specimen are the rounded bodies represented in Figs 1 to 6, and which have been spoken of above as micrococcus balls. They vary very greatly in size; the majority of those in the specimen from which Fig. 1 was taken measured from 0.15 to 0.375 mm. Many are not more than .0075 mm., while at Fig. 6 one is shown which measured .1500 by .1125 mm. In places they occur in hundreds, closely set together, and often very small, as at Fig. 4. The outlines are sharply defined, but it is not certain whether they possess a definite membranous investment. They contain minute refractile granular spherules, which behave with reagents like micrococci. In some of the larger balls, as shown in Fig. 6, secondary ones can be seen.

I am not prepared at present to discuss the nature and affinities of these structures, but hope to do so on another occasion, when I shall enter more fully into the histology of the primary and secondary lesions of this disease.

The vegetations in the other cases may be finally spoken of together, as, histologically, they presented the same features, with a few slight variations. A section through the grayish-yellow material composing the outgrowth has an appearance as represented at Fig. 7,—groups of granular bodies separated by an indifferent tissue. These colonies are usually closely compressed, and form rounded or tubular structures.

In the specimen from which Fig. 7 was taken, the groups measured from .050 to .125 mm. in diameter. In the outgrowths from the aorta in Case 5 this arrangement in colonies was particularly marked, and there were sharply-defined bodies, which bore a close resemblance to the micrococcus balls of Case 1, even to the occurrence of secondary spheres

within them. In a section through the entire thickness of an outgrowth from the mitral valve, 7 mm. in diameter, the following appearances were presented: At the site of attachment there was moderate proliferation of the endocardial tissue, as shown by numerous round and elongated corpuscles, which stained deeply in logwood. The greater part of the thickness of the mass is made up of irregular groups of dark granules, separated by indistinct fibrinous bands. The arrangement is not so uniform as that represented at Fig. 7. In the superficial parts the texture is lower, the fibrinous laminæ more distinct, and the corpuscles much more abundant. A collection of red blood corpuscles exists just beneath the outermost layer of this fibrin.

I do not propose to make any further remarks upon the special clinical features of these cases, none of which came under my care. My attention, however, has been directed to several circumstances in connection with the disease, which have not, so far as I know, received attention at the hands of writers on the subject.

First.—The fact that *primary infectious endocarditis* in the majority of cases does not occur in connection with acute rheumatism, as is almost universally stated to be the case. I have gone over the reports of 57 cases of this kind, and in only 15 is there any mention either of *acute rheumatism* or of previous rheumatic attacks, *i. e.*, in 26.3 per cent. I have not been able to make an exhaustive review of the literature of the subject, but have gathered the cases from the British and American journals, transactions, hospital reports, and from some of the recent French and German journals. I have excluded those due apparently to septic infection, as from whitlow, urethral laceration, acute necrosis, and the puerperal condition. Nor have I included those instances described as ulcerative endocarditis in chronic valvular affections (with dropsy, etc.), class 3 of above

division, often accompanied with aneurisms of the valves; but it may be mentioned in this connection that Dr. Ogle, in the ninth volume of the "Transactions of the Pathological Society of London," gives 21 cases of aneurism of the valves from ulcerative endocarditis, and of 18 of these cases in which a history is given, 15 are distinctly stated not to have had rheumatism. Kirkes,* the pioneer in this department of pathology, noticed the fact of its independent occurrence. I confess to having been considerably surprised at the result of this investigation, as I was previously of the opinion, expressed so strongly by Rosenstein† and others, that the great majority of the cases were met with in connection with acute rheumatism.

The second point to which I wish to direct attention is the frequency with which this disease occurs with pneumonia. Naturally, I regarded it as not a little remarkable that in five cases in succession I should meet with this combination. Cases 2, 3, 4 and 5 appear to have set in with the symptoms of ordinary pneumonia. In Case 6 it did not develop until after the patient had been in hospital for some days. In all, the disease appeared to be of the primary lobar form. In Cases 3, 4 and 5, at the time of the autopsy, the stage of hepatization had passed and resolution had begun. Of 21 cases of primary infectious endocarditis recorded in the "Transactions of the Pathological Society of London," hepatization of the lung is mentioned in 10 as a concomitant pathological condition. Of the 57 cases which I have analyzed, 22 were complicated with or occurred in pneumonia, *i.e.*, 38.5 per cent. What is the nature of this connection? Is the inflammation of the lung a complication of the endocarditis, or *vice versa*? In most of the cases it is distinctly stated that the lung was hepatized, and in

* *British Medical Journal*, 1863.

† *Ziemssen's Cyclopaedia*, vol. vi.

the majority of the instances the disease appears to have begun, as in Cases 3, 4 and 5, with the symptoms of ordinary pneumonia, so that the conclusion naturally suggests itself that the endocarditis was either secondary to the pneumonia or excited by the same cause, which latter I think the more probable supposition. Endocarditis is scarcely mentioned as a complication of inflammation of the lungs. In Huss' statistics* there are only 4 cases mentioned out of 959. Still, I was not altogether unprepared for the occurrence of the so-called diphtheritic inflammation in other organs in pneumonia. Bristowe† was, I believe, the first to point out that diphtheritic colitis was by no means infrequent in this disease, having met with it in 2 out of 30 cases of secondary and 4 out of 16 cases of primary inflammation of the lungs. I have also had my attention directed to this complication, though I have not met with it so frequently as Dr. Bristowe; still of some 40 autopsies in lobar pneumonia, of which I have notes, diphtheritic colitis occurred in 4, usually in the form of thin grayish-white patches, but in one case‡ there were large, thick, rupia-like masses involving the entire thickness of the mucosa. It is exceedingly interesting to note that in Case 6 this condition of the colon occurred with the pneumonia and endocarditis. Litten§ gives a case of ulcerative endocarditis accompanying diphtheritic colitis. The condition of the inflamed part of the lung in these cases did not present any coarse or microscopical differences from ordinary cases. There were no micrococci in the air-cells, nor any appearances resembling the remarkable bacteritic pneumonia described by Delafield.|| It is not very evident wherein the

* Quoted by Wilson Fox in Reynolds' System of Medicine.

† Path. Society Transactions, Vol. viii.

‡ Pathological Reports, Montreal General Hospital, No. 1, 1878.

§ Quoted in *Brit. Med. Journal*, Sept. 7, 1878.

|| Studies in Pathological Anatomy, Page 65, Pl. XXXV.

connection lies between these affections, but the very considerable number of instances in which they occur together is against a simple accidental complication.

A third point of clinical interest is the occurrence of meningitis in these Cases as in 2, 3, and 6. In the 57 cases which I have analysed this is mentioned as present in 13; *i.e.*, 22.8 per cent. In 7 it occurred with pneumonia. Meningeal hemorrhage, as in Case 5, is mentioned several times. It is probable that the meningitis is embolic, though I have not found micrococci in the exudation. Meningitis is a very rare complication of pneumonia and may occur apart from endocarditis; but in a case of inflammation of the lungs, particularly if the apex is involved (in 3 out of 4 such instances I found the upper part of the lung affected), the development of an irregular temperature with cerebral symptoms should suggest the possibility of endocardial mischief, with secondary meningeal inflammation. The exudation in the meninges in these cases is lympho-purulent, not very extensive, and generally on the surface of the hemispheres, not basic.

The presence of multiple aneurisms of the aorta in Case 5 is deserving of comment, as I have not been able to find any similar observation in the literature of either ulcerative endocarditis or of aneurism.

The man had evidently been the subject of that peculiar congenital malformation of the aortic semilunar valves which results in the fusion of two segments. In this condition they are very liable to be the seat of a sclerotic endocarditis which terminates in incompetency; and I have met with two other cases in which the united curtains, when in this state, were the seat of extensive ulcerative endocarditis.* The cardiac affection was evidently of old standing, and in

* On fusion of two segments of the aortic valves. Mont. Gen. Hosp. Reports, Vol. I, 1880.

February, 1879, a year and four months before his death, hypertrophy, a double murmur and a thrill were noted. The interest of the case centres in the four aneurisms of the arch, their age, and method of production. There can be no question of the recent character of the three small dilatations, but in the case of the large one there is room for doubt. Could it have been formed during the five weeks of his last illness, or was it of old standing, and was the thrill heard in February indicative of its presence? I incline to the belief that it was of recent origin for the following reasons:—1st. The character of the sac-wall, which was thin, in places translucent, looking like the stretched adventitia. In a very considerable number of aortic aneurisms of all sizes which have come under my observation, I have never seen one of this size with such a thin sac-wall and without any attempt at condensation. The internal and middle coats were not prolonged into the aneurism. 2d. The absence of laminated fibrin in the sac. Such a narrow-necked aneurism, if it had lasted for many months, would certainly have showed signs of the deposition of fibrin, which takes place in aneurisms quite as small and less sacculated. 3d. The condition of the intima of the arch. Apart from these aneurismal dilatations the lining membrane was remarkably free from degeneration, particularly when we consider the hypertrophy of the left ventricle which must have existed for some time. There was an entire absence of that *endarteritis deformans* which has, in my experience, been invariably associated with multiple aneurisms of the arch. 4th. A study of the four aneurisms shows that they have essentially the same characters and differ only in size. There is loss of substance involving the intima and media, the edges are covered with fungoid vegetations, and there is saccular distension of the adventitia, the only difference being that in the smaller aneurisms the breach of continuity

is slight, and the vegetations so luxuriant that they completely cover it. Whatever the essential nature of the so-called *ulcerative endocarditis* may be, I think there can be no doubt that in this instance we have to deal with an identical process in the arterial tube, which has caused loss of substance and subsequent dilatation, just as it does on the mitral or aortic valve with the production of valvular aneurism. If this be granted, Case 5 adds an interesting section to the etiology of aortic aneurism.

With regard to the intimate pathology of this disease, it is assumed by most recent writers to be a mycosis, *i.e.*, to be dependent upon the growth and propagation of lowly fungi on the valves with a consequent blood contamination. Certainly the minute bodies found in the endocardial vegetations correspond in their chemical and microscopical relations to micrococci. They are motionless, highly refractile spherules, less than a micro-millimetre in diameter, arranged in groups or colonies without any perceptible stroma. Acids, alkalies, ether and chloroform have no effect upon them. These characters are supposed to afford satisfactory means for distinguishing them from granular detritus of an albuminous or fatty nature. Most writers have accepted the view that these bodies are fungoid in nature. Heller,* however, criticizes strongly the prevailing conceptions with regard to micrococci, and thinks that there are scarcely any micro-chemical agents or physical signs by which they can be distinguished from fatty detritus. He recommends soaking the tissue in 10 per-cent. potash solution and then in iodine solution, 1 in 10 of spirit, which tints monads yellowish-brown, but is inert on fat granules. Sections of the vegetations in these cases, treated in this way, show the colonies stained of a brownish-yellow color.

* *Virchow's Archiv.* lxii, 1875.

Apart from any micro-chemical tests there are peculiarities about these masses which we do not see in any form of fatty degeneration, as the uniformity in size of the granules and their collection into large groups.

The question of the relation of the micrococci to the disease presents many difficulties, and we are probably not yet in a position to give a final answer to the problem. Klebs, and most German writers on the subject, give an unhesitating assent to the parasitic theory and suppose the micrococci to gain access either through the gastro-intestinal or respiratory systems, and they believe them to constitute the actual *materies morbi*. According to Koster* and Klebs† not only are these fungi present in the so-called ulcerative form, but they also exist in, and cause the development of, the ordinary warty or bead-like vegetations so frequently met with in the valves. Within the past few months I have examined four specimens of this variety of endocardial vegetation, and have been able to determine in each instance the presence of micrococci, not, it is true, in the same luxuriance, or arranged in definite colonies, but still sufficiently distinctive. In one case of mitral stenosis a fresh vegetation, when teased, showed many closely-packed spherules, some of which were, as Klebs has remarked with reference to the micrococci in this variety, larger than those met with in the ulcerative form. I was greatly struck with the resemblance which certain of these bodies, in this instance, bore to the individual elements of Schultze's granule-masses—those peculiar granular clumps common in blood of some animals and of impoverished persons. These structures are usually regarded as the *debris* of colorless blood corpuscles, but I have shown‡ that they are aggregations of discoid bodies,

* *Virchow's Archiv.*, Bd. lxxii.

† *Archiv für Exper. Pathol. u. Pharmacol.*, Bd., ix,

‡ Proceedings of the Royal Society, 1873.

probably living organisms of the nature of which we are still ignorant. They do not exist in the form of masses in the blood, but as isolated particles which might readily become adherent to the fresh endocardial outgrowths. I merely mention this as a point worthy of future investigation.

It matters little how the micrococci get to the valves, whether by embolism of the small vessels, as Koster supposes, or by deposition on the surface, as Klebs thinks; the question is: Are they responsible by their growth for the peculiar course and malignancy of cases of infectious endocarditis, primary or secondary? The facts of their occurrence in the verrucose form, which may not be accompanied by any symptoms, and of their abundance in the recurrent endocarditis, which attacks old sclerotic valves, are, I think, opposed to this view, for if they act as a septic poison in the one case, why should they not do so in the other? The micrococci do not appear to infest the blood in any numbers, so that they must be supposed to distil some subtle poison, "such soon-speeding gear as will disperse itself through all the veins" and profoundly disturb nutrition. The occurrence, however, of fatal septic cases, closely allied to, or identical with those in which a bacteric endocarditis is found, but in which no micrococci can be detected, either in the local process or in the blood, teaches us that the same poison may exist without the intervention of bacteria, the presence of which in any case may be only a partial phenomenon in a general infective process.

THE PHYSIOLOGICAL ACTION OF THE ACTIVE
PRINCIPLE OF PISCIDIA ERYTHRINA —
JAMAICA DOGWOOD.

By ISAAC OTT, M. D.,

MEMBER OF THE AMERICAN NEUROLOGICAL ASSOCIATION.

THROUGH the liberal enterprise of Messrs. Parke, Davis & Co., of Detroit, two powerful drugs have been introduced to the profession. These two agents are obtained from the island of Jamaica; the one urechites sub-erecta, a cardiac agent, resembling aconite in its action, the other a narcotic, whose action I shall speak of in this paper. In another place I have set forth some experiments made with a fluid extract of this narcotic. Through the kindness of Prof. Edward Hart, of Lafayette College, I am able to present some experiments (*Detroit Lancet*, June, 1880) with the active principle. He has worked out the active agent in this drug, and will fully describe its chemical relations.

Piscidia, the name by which I shall in this paper term the active principle, was suspended in water, or in glycerine and water. Its solubility in alcohol was of no value in my experiments, as the alcohol would have been a disturbing factor in studying the action of the active principle. It is on account of its insolubility in water that I am unable to give the exact dose the animal received.

GENERAL ACTION.

Experiment 1.—Rabbit, at 10.25 A.M., received about $\frac{1}{6}$ of a grain of piscidia, dissolved in alcohol, subcutaneously; no marked effect. 10.45 A.M., another $\frac{1}{6}$ of a grain given in the same manner; pupil at first contracted, afterward dilated. 10.52 A.M., incoördination, dozing. 11.50 A.M., another $\frac{1}{6}$ of a grain of piscidia given subcutaneously. 12.50 P.M., another $\frac{1}{6}$ of a grain injected into the jugular toward the heart; began to sleep, and continued to do so up to 3 P.M., when he fell over on his side; recovered during the evening.

Experiment 2.—Etherized rabbit received about a grain of piscidia, suspended in water, through the jugular toward the heart; in a few minutes convulsions ensued, rapidly followed by asphyxia and death.

Experiment 3.—Rabbit received through the jugular toward the heart about $\frac{1}{6}$ of a grain of piscidia. When let up, ran away; dyspnœa ensued, then sleepiness, drooping of head, till animal's nose touched the floor, when he fell over on his side; respiration became feeble; convulsions and death ensued; heart beat 244 per minute.

These experiments demonstrate that the active principle produces effects in no way differing from those caused by the fluid extract. It at first increases the respirations, produces incoördination, blunted sensibility, slowness of the heart, contraction of pupils, followed by dilatation, convulsions, asphyxia and death.

ACTION ON THE NERVOUS SYSTEM.

In this study I employed frogs of the variety without black spots—the “common green frog.”

Experiment 4.—Frog, at 8.10 A.M., received about $\frac{1}{10}$ of a grain of piscidia subcutaneously. 8.15 A.M., slight tetanus

of the posterior extremities; labored respiration; can be easily handled. 8.20 A.M., tetanus more marked. 9.10 A.M., tetanus well developed, with opisthotonos. 11.35 A.M., $\frac{1}{10}$ of a grain of piscidia subcutaneously; no tetanus present; considerable want of sensibility, which finally is completely lost; death at 4.10 P.M. Sciatic nerve bared and found to be irritable, the weakest current causing muscular contraction, being 240 min., du Bois' inductorium.

Experiment 5.—Frog, at 12.50 P.M., had the left iliac artery and vein ligated; he received about $\frac{1}{10}$ of a grain of piscidia subcutaneously. 1.10 P.M., the want of sensibility which precedes the tetanus equal in both posterior extremities. 2 P.M., same decrease of sensibility in both posterior extremities when tetanus was developed.

These experiments prove that piscidia tetanizes and abolishes sensibility, the want of sensibility being due to central causes, and not to an affection of the peripheral end of the sensory nerves. The great irritability of the motor nerves shows that they are not paralyzed.

SPINAL CORD.

When a small dose of piscidia is given to a frog, then convulsions ensue. To determine if they are cerebral, spinal, or muscular in origin is necessary.

Experiment 6.—Frog received about $\frac{1}{12}$ of a grain of piscidia. In about fifteen minutes convulsions ensued of a bizarre nature, medulla divided, but the convulsions still persist.

This experiment demonstrated that the origin of the convulsions was not cerebral. It does not prove that they are muscular.

Experiment 7.—Frog received about $\frac{1}{10}$ of a grain of piscidia subcutaneously; convulsions soon ensued. When medulla was divided, they still persisted; sciatic nerve on one

side divided, and when the muscles of that leg are tapped they become convulsed, although not to such a marked degree as is seen when they are in connection with the spinal cord.

The convulsions are partly referable to the spinal cord and to the muscles; they are of a spino-muscular origin.

In regard to reflex action, I have shown that the primary fall of reflex action is due to a stimulation of the inhibitory centres of Setschenow, the tetanus to a spinal and muscular excitation, and the subsequent loss of reflex action to a paralysis of the sensory ganglia of the spinal cord.

ACTION ON MUSCLES.

To study the action of this drug I employed the gastrocnemius of a frog and Marey's myograph, which registered the contraction of the muscle upon the drum of a Foucault regulator. The time of contraction was noted by a tuning-fork registering 240 vibrations per second. The tuning-forks were kept in activity by an electrical current. When the frog was in the convulsive stage he was bound down, his central nervous system destroyed, the gastrocnemius attached to the myographic lever through its tendon, and weighted to the extent of five grammes. The electric excitation was obtained from the secondary coil of du Bois' induction-apparatus, the primary circuit being broken by

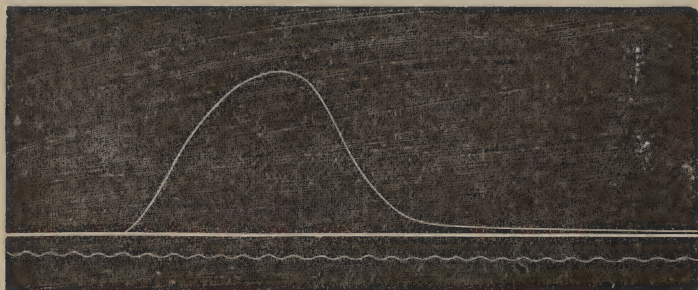


FIG. I.

the mercurial contact arrangement. The muscle was directly excited by thin wires, which were supported over it and did not interfere with the muscular contraction.

Fig. 1 represents the curve of a muscle not affected by piscidia, and taken under the same conditions as that of piscidia.

Fig. 2 represents the curve of piscidia muscle, showing the veratroid action of the drug on the muscle. Piscidia at first increases the height of a muscular contraction, and then reduces it. In both stages it causes veratroid contraction.

CIRCULATION.

In these experiments I used frogs and rabbits, the drug being suspended in water and injected toward the heart. In frogs it was used subcutaneously. The mean arterial tension was noted by means of Ludwig's kymographion. The time was noted by an electro-magnet and Ludwig's clock-apparatus to break the current every second. The pulse and pressure are given for periods of fifteen seconds.

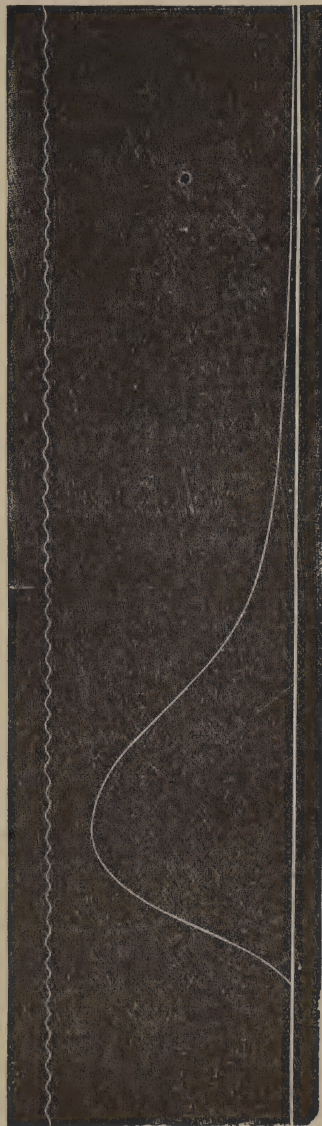


FIG. 2.

Experiment 8.—Rabbit, etherized.

TIME.	PULSE.	PRESSURE.
2.15. 0 P.M.	57	76
	piscidia, $\frac{1}{8}$ of a grain.	
2.15. 15 "	46	76
2.15. 30 "	50	78
2.15. 45 "	53	76
2.16. 0 "	52	76
2.16. 30 "	57	70
2.16. 44 "	28	64
2.17. 0 "	30	66
2.18. 15 "	22	76
2.18. 30 "	28	76
2.21. 30 "	30	76
2.25. 30 "	24	74

This experiment confirms the fact already noted with the fluid extract, that the pulse and arterial tension are reduced. To prove that this reduction was not due to stimulation of cardio-inhibitory ganglia, atropia was given, but it did not prevent the usual depression of the pulse.

Experiment 9.—Rabbit; atropia by the vein; etherized.

TIME.	PULSE.	PRESSURE.
3. 8. 0 P.M.	45	44
	piscidia, about $\frac{1}{8}$ of a grain.	
3. 8. 15 "	37	44
3. 8. 45 "	40	36
3. 9. 45 "	41	34
3.10. 45 "	43	34
3.12. 0 "	piscidia, about $\frac{1}{8}$ of a grain.	
3.16. 0 "	42	44
3.17. 0 "	47	44
3.17. 15 "	about $\frac{1}{8}$ of a grain of piscidia.	
3.17. 30 "	50	44
3.34. 0 "	36	65

Experiment 10.—Rabbit, etherized, received through the jugular twenty drops of the fluid extract of piscidia ery-

thrina, which rapidly reduced the frequency of the heart ; artificial respiration kept up ; chest opened. Then a solution of atropia was injected by the jugular, but the heart was not accelerated in frequency by it.

Experiments on frogs also confirmed the view that piscidia did not slow the heart by irritation of the inhibitory apparatus of the heart. A is the frog who previously received a dose of atropia ; B the normal frog ; chest opened in each.

TIME.	A.	TIME.	B.
9.20 A.M.	44	9.18 A.M.	38
9.21 "	$\frac{1}{10}$ of a gr. of piscidia subcutaneously.	9.20 "	$\frac{1}{10}$ of a gr. of piscidia sub- cutaneously.
9.22 "	44	9.21 "	34
9.26 "	38	9.25 "	34
9.29 "	36	9.28 "	34
9.35 "	34	9.30 "	34
9.45 "	32	9.32 "	32
9.50 "	32	9.36 "	34
10. 0 "	32	9.45 "	32
10.20 "	30	9.53 "	30
10.25 "	32	10. 0 "	30
		10.20 "	28
		11.25 "	32

That this drug does not paralyze the pneumogastric is proved by the following experiment :

Experiment 11.—Etherized rabbit received about $\frac{1}{8}$ of a grain of piscidia through the jugular ; chest opened ; vagus bared and irritated, when the feebly and slowly-beating heart was quickly arrested.

All these experiments, and those previously made with the fluid extract, show that the piscidia reduces the pulse and arterial tension ; that it does not excite or paralyze the

cardio-inhibitory apparatus. From the fact that this drug affects striated voluntary muscular structure, it probably also affects cardiac muscle, rather than the cardio-motor ganglia. The slight preliminary rise and subsequent fall of arterial tension are due to stimulation and paralysis of the monarchical vaso-motor centre, and paralysis of the heart itself.

Fig. 3 represents the normal curve of a rabbit's heart, registered by the mercurial kymograph, whilst Fig. 4 shows the same under the action of the piscidia. The topmost curve in Fig. 3 shows the commencing slowness of the heart under piscidia. The abscissa line is twelve millimetres below the base of the line denoting seconds.

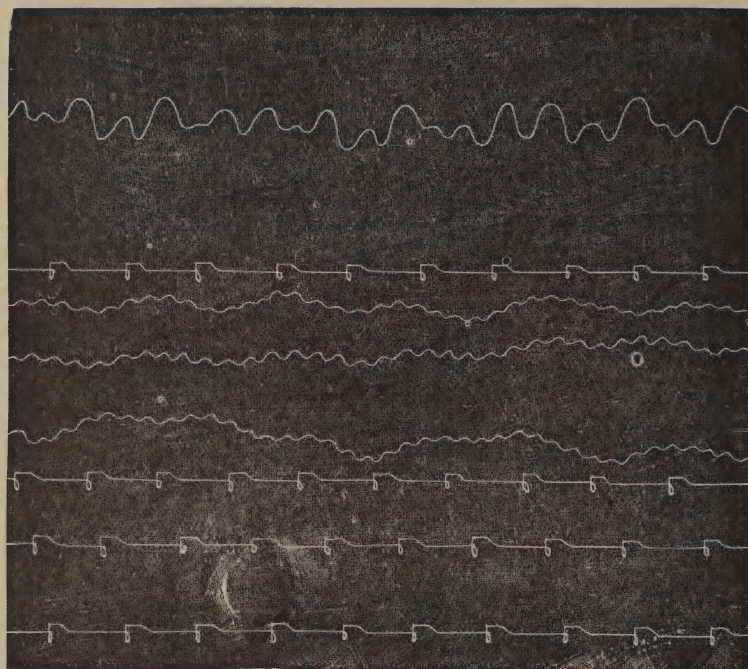


FIG. 3.

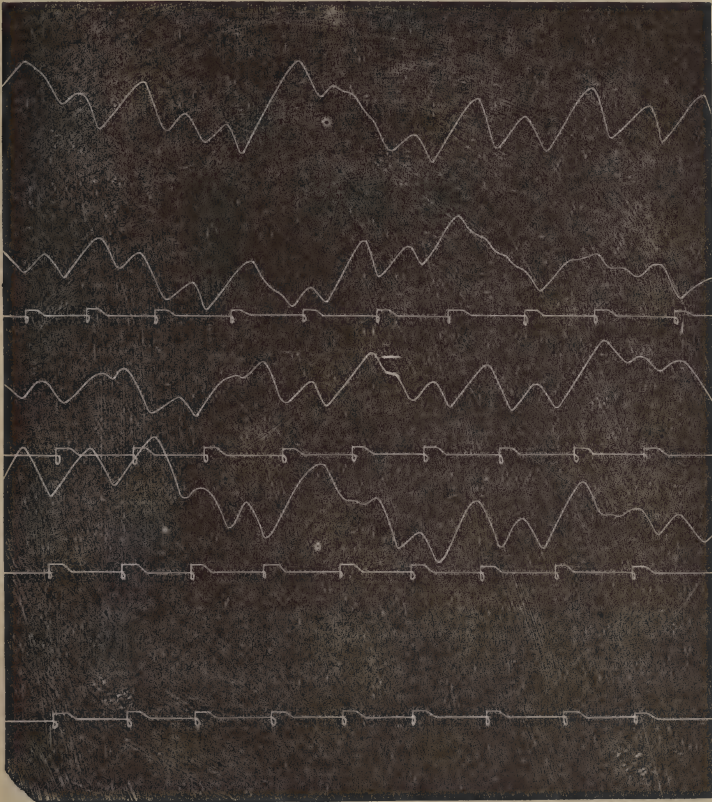


FIG. 4.

ACTION ON THE PUPIL.

Experiment 12.—Rabbit.

		WIDTH OF PUPIL.
12 M.,		8 mm.
12. 1 P.M.,	20 minims of fluid extract of piscidia erythrina subcutaneously.	
12. 5 "		8 "
12.10 "	twenty minims of fluid extract.	
12.25 "	" "	
12.45 "		8 "
1.32 "		7 "
1.50 "		7 "

		WIDTH OF PUPIL.
2. 0	P.M.	7½ mm.
2.10	"	8 "
2.15	" labored respiration.	8 "
2.24	" "	10 "
2.27	" convulsions in posterior extremities ; none in the anterior.	
2.35	"	11 "
7.50	" animal breathing normally ; no con- vulsions ; cannot voluntarily move his limbs.	12 "
12 M.	next day, animal running about.	10 "

The drug at first contracts and then dilates the pupil.

These experiments have fully confirmed the results obtained from the fluid extract of *piscidia erythrina*. The following results may be drawn from these experiments :

1. That *piscidia* is a narcotic.
2. That it does not paralyze or excite the motor nerves.
3. That it does not act on the extremities of the sensory nerves, but their central connection—the sensory ganglia of the spinal cord.
4. That it produces convulsions, partly by stimulation of the spinal cord and partly by heightened excitability of the voluntary striated muscles.
5. That it reduces the frequency of the pulse by an action on the heart itself, probably on its muscular structure.
6. That the arterial tension temporarily rises by stimulation of the monarchical vaso-motor centre ; that it soon falls, due to a partial paralysis of this centre and the heart itself.
7. That it at first contracts and then dilates the pupil.

EDITORIAL DEPARTMENT.

NON-RESTRAINT IN THE TREATMENT OF THE INSANE.

By J. C. SHAW, M.D.,

MEDICAL SUPERINTENDENT OF KINGS COUNTY LUNATIC ASYLUM, FLATBUSH, N.Y.

It is now one year since we adopted non-restraint at the Kings County Asylum, and nine months since, by invitation, I read my paper on that subject before the National Conference of Charities, at Cleveland, Ohio. We have since that continued to carry out the system in the face of the many difficulties which surround us, and can to-day confidently say that we do not regret its adoption, and see no reason for returning to the use of restraint ; in fact, we have seen much to commend its continuance. We have endeavored to carry it out faithfully, observingly, and with firmness, so that we might judge of its applicability justly ; not attributing, without careful examination, every little annoyance and trouble or an accident to its adoption, but taking ourselves to task, examining the thing carefully, and always finding out that the blame did not attach to the want of restraint apparatus, but admitting to ourselves candidly that it was due to a want of care on our part, or from a non-appreciation of the care necessary. As soon as this unforeseen defect appears, we remedy it and find no further trouble, but do not at once conclude that a return to the apparatus is demanded. This, and this alone, is the spirit in which the system of non-restraint should be approached. I think myself justified in saying that it is not the system which is at fault, but those who adopt it and fail.

The adoption of it is demanded from a humanitarian point of view ; patients beg to have the apparatus taken off of them. Those who wear the camisole night and day are placed in a most uncomfortable and constrained position. It is hard to conceive how a patient can sleep in one of these things ; the struggle which the patient has with the attendants to get the apparatus on, inflicts injuries, bruises and humiliation on the patient. If he is powerful, and the attendants have a hard struggle with him, they will often use undue force ; and if not having a correct appreciation of their calling—which they seldom have—they may, and do, try to bring the patient to a quiet state by striking him, kneeling on him, etc., and very often the restraint is applied without just and sufficient reason. The result is that hereafter the attendant and patient are in antagonism, which is detrimental to the comfort and health of the patient. All this is infinitely worse than any trifling accident which might occur from the want of restraint, but this is entirely lost sight of by those who argue against non-restraint. It has been said that patients would rather be in restraint than be handled roughly by the attendants ; that may be true, if such rough handling were required, but it is not. If you have a patient who has to be so handled by the attendant, place him under the care of another, if you have any good attendants. You will surely find one that the patient gets along with ; adapt the attendant to the patient, then you will find such argument useless ; if patient and attendant disagree, and which always occurs when the attendant has to use restraint apparatus on the patient, you will always have trouble with them both as long as you keep them together, unless your attendant, by brutal treatment and beating, compels his charge, thereby, to keep quiet and submissive.

There is a class of patients which, it is believed, ought to be in restraint : those who have hallucinations and delusions of a terrifying character. Restraint simply adds to the terror of these patients ; these are the patients which are often put in “cribs.” Patients admitted to asylums are often frightened by the strange sights, sounds, and unfamiliar faces which they see and hear. If

they believe that they are to be destroyed, killed, or tortured, these surroundings and the restraint strengthen or add to their terror.

Where restraint apparatus is used freely, it is simply encouraging attendants to neglect the care of the patients. Attendants, ordinarily, like nothing better than to have restraint used, and they will advocate its use to the physician. When you find an attendant asking to be allowed to use restraint, it is evidence that the attendant is either desirous to save his or herself trouble, or that he does not know how to care for the patient.

No one knows better than I do the care and anxieties connected with the position of superintendent of a large asylum. In some asylums the insufficiency in the number of attendants, and, above all, the want of occupation for the patients, are some of the difficulties in the adoption of non-restraint, but in spite of these difficulties much can be done.

Those who are opposed to non-restraint are in the habit of parading out a long list of casualties which have occurred in asylums without restraint, but ignore the accidents which occur in asylums with restraint. To judge of the benefit of the system by such a test is unjust, and is an argument which would only be brought forward by men determined to defend the practice at all hazards.

Dr. Allan McLane Hamilton, in his testimony before the Senate Committee, was a defender of restraint, and he made the statement, which was partly incorrect, that a patient committed suicide at the Kings County Asylum because she was not put in a "crib."

Dr. Hamilton is a blind defender of the "crib" and other restraint apparatus, and advocates its use at all times and on all occasions. The patient who committed suicide at the Kings County Asylum was a quiet melancholic. Now to argue that this patient ought to have been put in a "crib" is ridiculous and unjustifiable, and in the use of this argument Dr. Hamilton has placed himself on record as a defender and advocate of the neglect of the personal care which patients in such a condition ought to re-

ceive. The adoption of restraint in such cases means simply neglect of the patient, and he could not have used a better argument in support of the position we take.

The use of restraint apparatus means the neglect of the patients by the attendants and medical officers ; such patients ought to be under the eye of attendants night and day ; that is the remedy for suicides, and not the "crib."

It is strange that the restraint system is defended so energetically by a comparatively few superintendents, the others acquiescing.

Dr. Andrews, formerly at Utica, a bright, energetic gentleman, on a visit made by us to the Utica Asylum, told us that they used very little restraint, and we really saw none except the "crib." We saw violent patients having a good deal of liberty, as they ought to have.

Dr. Carlos McDonald, who testified before the Senate Committee, admitted that he had tried non-restraint and found no harm from its use, but appears to have abandoned it without good reason. After such an admission, he at once proceeds to argue in favor of restraint. It is evident from what we have seen and heard from Dr. McDonald, that his defence of restraint is not so from personal conviction.

Why this inconsistency ? Why are these superintendents so pertinacious in defending a system which is so widely adopted in America to the detriment of patients, when they themselves admit that they use very little of it, or pretend they do ?

What motive can they have in making themselves the champions of asylums that use it unjustifiably ?

This is, to say the least, a strong proceeding.

The question is frequently asked : What do you do with patients who break glass windows, etc. ?

This leads me to say a few words in regard to the method we adopted at the start, and that which has been our guide up to this time.

Whoever has been much in an asylum knows that a variety of small things are continually coming up, which have never

before appeared in the same way. Many of these have to be left to the good judgment and discretion of the medical officers, as patients have often to be treated quite differently, and this calls for a special decision. An outline of the plan we adopt and are guided by can, therefore, only be given.

At the beginning we sought to find out why such patients were in restraint. We often found what was given as a reason was, in our judgment, quite inadequate, the nurses stating that they are bad patients and cannot be got along with without restraint. So we change them to other halls with other nurses, and then remove the restraint, and we found no trouble. Have no restraint apparatus ; then not having it, you are compelled to find some other way of getting the patient to behave properly. Having the restraint apparatus, you and your attendants are very apt to resort to it and think it necessary, and could not get along without it ; but if you have none you will soon find that it is unnecessary. I cannot do better than give an illustration of how we manage.

A young man is admitted ; he goes to a reception hall ; is quite excited ; as soon as he finds he is in an asylum, he is determined to get out ; he becomes more excited ; he suddenly rushes against the door with all his might, striking the door with his shoulder ; he breaks the door open, tears the lock out, and splinters the door in several places ; he finds himself in another hall, into which the door opens ; he sees and is told that he cannot get out ; he soon becomes calmer, and does not try again to get out. Ordinarily that patient would have been put in a camisole at once. A grand struggle would have ensued, and perhaps damage done to some one. The carpenter mends the door. We seldom have anything worse to deal with in the male department. If a patient breaks a door, a glass, or more, we repair the damage and leave the patient alone. When patients do these things they usually do so at once, unexpectedly, and that is the end of it. They do not do it again, especially if left alone ; it is no use putting them in restraint after it is done.

A patient will sometimes strike another ; this is generally because some difficulty arises between them ; he strikes the patient

and that is the end of it. Restraint is no prevention of this, unless you keep them all in restraint ; it is better for the attendant to try to prevent this. It will sometimes occur in spite of ordinary care, and the patients will themselves explain the difficulty to you. I have just had an affair of this kind occur, which illustrates what we sometimes do. A man, who for months past has been peaceable (he has always been rather pugnacious), has been in a hall where all the patients work out-of-doors winter and summer ; it is found that he begins to become quarrelsome, wants to fight with the other patients ; but there are three men that he appears particularly anxious to have a pugilistic encounter with. This disposition he presents for many days, and on one day strikes one of them ; he is surrounded with quiet men. It is evident that at present this is not the place for him. If I leave him here there will soon be a grand row. I at once have him removed to another hall. This is the remedy.

Those patients who tear up clothing it is most difficult to deal with, as far as the prevention of this propensity is concerned. In the majority of cases, however, this habit lasts only a certain length of time, and then is given up. It is seen mostly in paretics. So far I have allowed them to tear up. In a few instances, we have tried to keep them occupied in doing something else, if their physical state permitted it. I have had persons express horror and surprise at my allowing patients to tear up things, but they did not choose or stop to think that it is infinitely worse to have the patient suffering in restraint. We have tried, with some success, quilting blankets with burlaps.

After all, what we have lost by tearing up is comparatively small.

The above-cited class of cases are the ones you oftenest see in restraint in asylums, and which swell the percentage of patients in restraint, and not those who come in a violent maniacal state, complicated, perhaps, with epilepsy. These are the cases which, in my opinion, require medication rather than the camisole. These are, however, not a class of cases which help to swell the population of an asylum. Seclusion may be necessary with such

a case. We have had very seldom to resort to seclusion in the male department.

We have had a little different thing to deal with in the female wards. Some of our most noisy and quarrelsome patients never require any interference. A few cases of periodic mania give us some trouble. When in the maniacal state they will fight with each other unless watched constantly, and even then they will sometimes begin a fight and require to be separated by the nurses. It occasionally happens that under such circumstances I am obliged to order a female patient into seclusion for a couple of hours at a time. I have never had to carry it out for longer than six hours at one time. A great deal is in the adaptation of the nurses to the kind of patients you have to deal with.

There may be two excellent nurses. One of them thoroughly unfit to take care of a violent patient, and the other one admirably adapted for it. It is impossible to get a corps of nurses all equally good.

The style of buildings we have for asylums is not the best for non-restraint. The halls are too large and long; too many patients have to be kept together (this often gives trouble); there should be large and small wards, some wards so small as to hold only six to twelve patients. The crowding of a lot of patients together is detrimental to them, and makes it more difficult to care for them properly. Thirty-five to forty patients in one ward are a great deal too many. It may be advantageous when trying to conduct an asylum in the cheapest manner possible, if that is all that is aimed at in the care of the insane.

The changes and advantages apparent since we adopted non-restraint, may be summed up as follows: Less discontent, fewer encounters between the attendants and patients; we hear less often of patients breaking glass.

We have fewer fights between the patients themselves; they do not so often importune to go out, and I think I am safe in saying that we really have less destruction of clothing and furniture. We do not meet with the bruises inflicted by the apparatus and the application of it, and the unfortunate patient importuning to have it taken off, and we see no indication for its use at this day.

NEW BOOKS AND INSTRUMENTS.

A Treatise on the Practice of Medicine for the Use of Students and Practitioners. By ROBERTS BARTHOLOW, M.A., M.D., LL.D., Professor of Materia Medica and General Therapeutics in the Jefferson Medical College of Philadelphia. New York : Appleton & Co., pp. 853.

It has often been a matter of surprise to us that there were so few American text-books on the Practice of Medicine. The works of Wood and Flint stand alone in this branch, and have been before the medical public for thirty-four and sixteen years respectively. These, with the numerous foreign reprints, have doubtless more than supplied the demand of students and practitioners. Niemeyer's work is as well known here as in Germany, and the text-books of Tanner, Aitken, Roberts, and Bristowe, have large sales. Still, the fact that for nearly twenty years no native author has appeared on the field, speaks loudly for the modesty of the sixty-five professors of "Theory and Practice" in this country; for the temptation to a popular teacher in a large school to figure as an author in his department is very great, and a circle of admiring pupils and friends is always at hand to urge the task upon him. To some such pressure, indeed, Dr. Bartholow tells us that he has yielded, but we do not think that his friends, or the profession in general, will regret this, for he has produced a work which will enhance his own reputation, and sustain that of American text-books of medicine.

The extremely moderate dimensions of the volume will at once attract attention in a work professing to be a treatise on the subject. It is not much larger than one of the volumes of Niemeyer. On looking through it we find that this has been accomplished by the omission of chapters on general pathology, semeiology and

etiology, and by a general condensation of the remaining subjects. The absence of the former is not much to be regretted, as special works on pathology are now easily accessible, but the student will miss the introductory chapters on clinical characters, method of examination, etc., which, in many text-books precede the consideration of groups of local diseases, and are, we think, of great service to him.

The work is divided into two sections, viz., special pathology and therapeutics, and general or constitutional diseases; the former dealing with the affections of the organs of the different systems in order, beginning with those of the alimentary canal.

The sections on diseases of the stomach are well written, though there is nothing in the description of the pathology or symptoms which differs materially from the accounts in other text-books. Attention is called to the distinctions between the acute perforating and the chronic indurated ulcer, and it is stated that facts are wanting to demonstrate a transitional condition between them. We must take exception to the statement that a copious hemorrhage never takes place from the stomach by diapedesis alone; not only may it do so, but it may persist and prove fatal—particularly in cirrhosis of the liver—without a trace of ulceration of the mucosa or the existence of a thrombus in the portal vein. The copious, often fatal, hemorrhages from the stomach which occasionally occur in leucocythemia and in splenic anæmia, are the result, in our experience, of diapedesis. The treatment of the various gastric disorders is full, and, for the most part, satisfactory. The author has great faith in the virtues of arsenic, and recommends it strongly in chronic catarrh, ulcer, and cancer, chiefly for the pain, but in the latter disease, on the supposition that it may, if of the epithelial variety, retard the growth. Scarcely sufficient stress is laid on the value of morphia, as pointed out by Stokes, in gastralgie attacks, whether depending on ulcer or not. In our hands no remedy has been so successful, and given by the mouth it appears more beneficial than when administered hypodermically. In the symptoms of dilated stomach, these important features, present in very many cases, are overlooked, viz., the characteristic outlines of the dilated organ, seen through the thin abdominal walls; the energetic vermicular contraction, and the existence of an evident tumor at the pylorus. The stenosis causing the distension not unfrequently follows the cicatrization of an ulcer, and even when cancer is present the symptoms of dilatation may be the most prominent.

Under the diseases of the intestines, there is a capital account of typhlitis, a subject which in many text-books does not receive due attention. In the treatment of dysentery, sulphate of magnesia and ipecac are given the first and second places, and Fowler's solution with opium has yielded good results in the author's hands. As is currently believed, the *tænia solium* is stated to be the common tape-worm of the country; we think, however that the unarmed variety, or beef tape-worm, is the most prevalent, as in England. We are glad to see that Dr. Bartholow has restored to it the original name, given by the Pastor Gœze, of T. saginata.

As supplementing the excellent account of contracting cirrhosis, we should like to have seen a description of the hypertrophic form, which differs so strikingly in many of its features, and is certainly common enough to have received attention. In the prophylaxis of gall-stones, the author speaks very confidently of careful regulation of the diet, excluding fatty and saccharine matters and reducing the starchy ingredients, and says, moreover, that, when present, the administration of sodium phosphate, in drachm doses, three times a day, if continued for several weeks or months, will almost always cause the solution of the stones.

The interesting subject of the treatment of valvular diseases of the heart is dealt with in a full and satisfactory manner, and the indications for iron, digitalis and purgatives clearly laid down. A few remarks on some of the contra-indications of digitalis would have increased the value of the section. Where this drug cannot be borne *cimicifuga* is advised as a substitute.

In the treatment of pneumonia there are one or two points which have scarcely received proper consideration. The expectant method is not alluded to; now, we fully believe with Niemeyer that simple pneumonia, uncomplicated, requires no active treatment, and at least two-thirds of the cases do well without any medication. The reaction in favor of bleeding has evidently not touched Dr. Bartholow, as he passes over this important matter in silence. We are firmly convinced that many patients die annually of this disease who might be saved by a timely bleeding. We were led to this opinion by experience in the dead-house. Bodies of strong, full-blooded young men have been sent down in which the morbid picture was that of asphyxia; hepatization of the greater part of a lung, engorged right heart and venous system, healthy parts of lung congested and œdematous. In some of these cases it has simply been a matter of pulmonary obstruc-

tion, the issue of which was a suffocative œdema. We have had clinical experience of the importance of bleeding in this condition, and are persuaded that neither stimulation nor other medical means can be employed as a substitute.

The subject of phthisis is treated of under three heads: Caseous, Tubercular, and Fibroid Phthisis; perhaps for the student, to whom the unsettled state of this question is a real grievance, and who is bewildered by the diverse theories at present prevailing, this is the most satisfactory arrangement. The author is evidently not in sympathy with the reviving belief in the unity of all the forms. We should like to have had Dr. Bartholow's opinion on the communicability of the disease, and the infective theory, as advocated by Cohnheim and Creighton.

We are glad to see that the author is among those who maintain that croup is "an independent, substantive disease."

The section on diseases of the kidney is carefully prepared and well up to date. We were surprised to find a separate chapter on that rare affection, hydatids of the kidney; the subject of hydatids of the liver and lungs is also considered at some length. We have not been able to gather what the writer's experience has been of the frequency of this disease in America. Our impression is that echinococcus cysts are rare in this country, even in the liver, much more so, at least, than in Europe.

Cerebral affections naturally come in for the attention which their importance demands, and the descriptions of individual diseases are full enough for practical purposes. A more extended account of the diagnosis of lesions might have been expected, and no attempt is made to follow up the subject of cortical localization. In the affections of the spinal cord, an introductory chapter on the general pathology and localization of lesions is wanting. It would have been of great service to the student, and would have enabled him to appreciate more fully the subsequent details. In locomotor ataxia no mention is made of syphilis, which is believed by Gowers and others to be an important etiological factor. Among the cerebro-spinal neuroses, epilepsy and hysteria receive ample attention, but there is nothing novel in the details of symptoms or treatment.

The description of the eruptive fevers is good, and we are glad to see attention drawn to the rather common initial rashes of small-pox, which are often serviceable in diagnosis. Vaccination is briefly discussed and the use of bovine lymph is recommended in preference to the humanized. One naturally turns in

a work by a new author to the important subject of the treatment of typhoid fever. Dr. Bartholow speaks favorably of the specific treatment by iodine, but prefers to combine it with carbolic acid. He is not very enthusiastic about the cold-water method, and prefers quinine as an antipyretic. A note to the article on this section states that typho-malarial fever does not exist as a separate entity, but is simply typhoid modified somewhat when occurring in a person saturated with malaria. It is to be hoped that the use of this term will now be abandoned, particularly as Dr. Woodward, who introduced it, has retracted his original observations on the subject.

Diphtheria receives an extended notice. The author is not very positive about the relations of the micrococci to the disease, but is inclined to think that they enact a secondary rôle. The paralyzes are well described, but the account of the serious heart lesions is somewhat meagre. Lactic acid and insufflation with sulphur are highly recommended as local remedies. In the treatment of acute rheumatism, the author appears to favor the full alkaline method of Fuller, and we think that the salicylates have scarcely received the justice which their merit demands.

Altogether, the impressions which we have received of this work as a whole, are very favorable, and we venture to predict for it considerable popularity among students, though in many respects it is inferior to Roberts' hand-book for beginners. One misses that feeling of the personality of the author which shines out so conspicuously in Niemeyer's text-book, but glimpses of it appear here and there, particularly in the sections on treatment. Throughout the work we have ample illustration of the author's assertion in the preface, that he has no sympathy with the therapeutic nihilism of the day. It is cheering, indeed, to meet with such faith as is expressed on p. 501, where carbonate and iodide of ammonium are advised in the treatment of thrombosis of the cerebral vessels, on the supposition that a solution of the thrombus may be effected by maintaining the alkalinity of the blood, and a similar conviction is expressed on p. 179, where the intravenous injection of ammonia is urged for the purpose of dissolving a thrombus in the portal vein. (w. o.)

A Practical Treatise on Fractures and Dislocations.

By FRANK H. HAMILTON. A. M., M. D., LL.D., Surgeon to Bellevue Hospital, New York ; Consulting Surgeon to the Hospital for the Ruptured and Crippled, etc. Sixth American edition. H. C. Lea's Son & Co., Phila., 1880, pp. 899.

It may seem that a new edition of a work so well and favorably known as this, needs nothing more in the way of a review than a repetition of the ordinary stereotyped phrases of commendation which have been so often indiscriminately showered upon it. As Dr. Hamilton states in his preface that one entirely new chapter has been introduced, that another has been "entirely rewritten," and that "most of the chapters have undergone thorough revision," this present edition, in many respects, should be viewed as a new work, which must stand or fall by its own merits. After a most careful perusal of this edition, we are ready to admit that a certain amount of revision has been practised with regard to portions of the book, but we must express sincere regret that it was not carried further, so that in all respects the work might be, what it is in great part, the best treatise in any language upon fractures and dislocations.

The first portion of the work treats of fractures for 580 pages, divided into 35 chapters; the remainder, viz., 319 pages, is devoted to the consideration of luxations, both traumatic and congenital, and is divided into 26 chapters.

To give to our readers anything like an exhaustive critical analysis of such a large work as this would far exceed the limits assigned to us. We shall, therefore, be compelled to call attention merely to those points which seem most clearly to demand either our praise or blame.

This latter—blame—may seem uncalled-for by some of the ardent admirers of Dr. Hamilton, who can see no faults in his works. Let them comfort themselves, however, in the present instance, by recalling the fact that to an intelligent reader indiscriminate praise is worse than the faint praise which damns, so that our strictures will positively enhance the author's reputation, acting as a condiment which renders palatable and digestible what might otherwise prove cloying to the mental palate.

To commend is so much more pleasant than to find fault, that we will first bestow unqualified praise upon certain statements in the chapter upon general semeiology and diagnosis. If all surgeons of repute would not only admit to themselves, but teach openly what the subjoined sentences do, we should have fewer suits for malpractice. "We cannot, in a pretty large proportion of cases, bring the broken ends again into apposition. Whatever mere theorists may say to the contrary, and notwithstanding surgeons up to this time have rarely ventured to allude to this subject, the fact is that we rarely do usually 'set' broken bones.

We do not, even at the first, bring them into complete apposition, unless it is as the exception. I speak of the bones once completely displaced by overlapping, and these constitute the majority."

"Setting," the author defines to be the placing of the fragments end to end, so that they may be made effectually to support one another. We cannot endorse too strongly the advice given to handle fractured limbs with the utmost gentleness, and we would add, when the surgeon has once elicited sufficient evidences of a fracture, he should be satisfied, and not try to make "assurance doubly sure" by prolonged examination. If anæsthetics be used, especial caution must be exercised during the manipulations, as pain no longer acts as a danger signal to prevent the use of undue force.

The opening part of Dr. Hamilton's chapter on the repair of broken bones is lamentably weak where he speaks of "plastic lymph," etc. We cannot admit the excuse as valid that this subject "more properly belongs to the general treatises on surgical pathology." Where should we look for special and minute teaching but in a special work on fractures? In the next edition we trust that this antiquated chapter will *really* be "revised."

The chapter on general prognosis is so good that we wish that we could quote from it freely. The only points which we wish to call special attention to, are the nearly constant occurrence of some shortening,—Dr. Sayre's statements to the contrary,—and the published observations of a number of gentlemen which demonstrate asymmetry of the lower extremities in many individuals. To certain of our readers the astounding statements made by Dr. Sayre, in the following words, may be unknown, so that we will quote them that our comments may be more readily understood :

"Fractures of the long bones require that *extension* and *counter-extension*, under the influence of *chloroform* or other anæsthetic, if necessary, should be made in a *proper direction*, until perfect accuracy of adjustment is obtained, and, after this, *retention and fixation in this normal condition* until *consolidation*. (The italics are Dr. Sayre's.) By accuracy of adjustment, I mean the perfectly normal condition of the bone as to length and position. When the extension and counter-extension have been properly made, the muscles and other tissues surrounding the bones will necessarily and positively force the fractured extremities into their natural position, as above described, unless some foreign body, as a shred of muscle or connective tissue, has got between the fragments."

If Dr. Sayre's views are correct, it is astonishing how almost universal the rule is for "shreds of muscle or connective tissue to get between the fragments" in *the practice of every other surgeon besides Dr. Sayre*. Fortunately, Dr. Sayre gave a table of cases in support of his views. Fortunately, we say, because Dr. Hamilton gives us an able analysis of them, showing that in the femur but three cases were *not* shortened (one was seven years, one seventeen, and here the *fragments had never been separated*, and one twenty-three years). The others had shortening of from one-fourth of an inch to two inches.

In the leg and arm fractures, after having excluded compound fractures, and those of the tibia or fibula *alone*, where no one expects shortening, but *one case is given where measurements were made*.

Dr. Hamilton finally gives an account of one of his own cases treated with plaster of Paris, where both he, Dr. Krakowizer, and others, found a shortening of one inch, but Dr. Sayre considered the limb to be *lengthened*. After such statements Dr. Sayre's statistics will hardly be thought very reliable by most surgeons. We have devoted so much space to these statements of Dr. Sayre, because an admission of the truth of such extreme views would render suits for malpractice of daily occurrence. Dr. Sayre's paper has been quoted as authoritative and final in a recent work on medical jurisprudence. Its author would have shown himself a better surgeon by having endeavored to combat such views instead of having quoted them at the great risk of serious detriment to members of the profession. His ready adhesion to such unsurgical views demonstrates his own want of surgical experience. It has only been quite recently that the asymmetry of the extremities has been demonstrated, but the important bearing of this fact upon the result of the treatment of fractures can be seen at a glance. It is quite likely that in certain cases where no shortening of the femur ensues upon its fracture, in reality the limb *is* shortened, but being originally the longer one the tape line now shows both extremities to be of the same length. When the shorter limb is the one injured it can be readily seen how the surgeon may get all the credit of the difference in measurement, whereas nature really deserves most of the blame. When it is stated that the normal difference between the limbs has been sometimes considerably over half an inch, without having challenged the slightest observation, one can easily see how important such knowledge is, and how leniently we should judge of re-

sults in the practice of others, where great apparent shortening has resulted.

Chapter VI, which treats of the "General Treatment of Fractures," is worthy of more care in reading than we imagine is usually bestowed upon it. Here the author endeavors to show what are the general principles involved in the treatment of all fractures, and that the indications can be fulfilled in numberless ways. We join him heartily in condemning all kinds of carved and moulded splints which are simply "royal roads" to bad results. With such apparatus—which the practitioner thinks needs only be bandaged to the limb to ensure a good result—endless trouble and evil is likely to result, while a man who is thoroughly imbued with the general principles governing the treatment of fractures, will secure an admirable result with nothing beyond the heel of a loaf of bread perhaps. Dr. Hamilton's remarks upon immovable apparatus we consider eminently judicious. Wherever such injury to the soft parts exists, as to render great swelling and inflammation likely, he interdicts their use as too dangerous even with the exercise of the utmost care. Again, they are quite as likely to become too loose as too tight. Dangerous sloughing and gangrene may take place under cover of such dressing, without necessarily producing such marked pain as to warn a careless surgeon. He says: "At the present moment the use of plaster of Paris as a dressing for fractures is very little in favor with most of the Bellevue surgeons, except in fractures of the tibia and fibula." As these gentlemen have been in times past the great advocates of this method, such a statement is very significant. Dr. Hamilton thinks "that the 'apparatus immobile' constitutes an invaluable surgical appliance" in certain rare transverse or serrated fractures where no displacement has ever existed, and where, in consequence of the slight injury to the soft parts, little or no swelling will ensue, as well as in certain more complicated ones where some union has occurred with subsidence of the inflammation, swelling, etc. It is specially adapted to cases of delayed union. But all this commendation is only with the provisos that the bandage be but moderately tight, that a part of the limb be left uncovered for inspection, and that the dressing be entirely removed from time to time. In the later stages of certain compound fractures he would also sometimes use it. We should hardly like to meddle with a compound comminuted fracture, where the limb is to be saved, by attempting to remove partially-attached fragments, as Dr. Hamilton advises, but we do

agree with him as to the danger of sealing the wound where much contusion of the soft parts renders a marked degree of inflammation inevitable. In appropriate cases, sealing rapidly converts such cases into simple fractures. Dr. Hamilton makes no mention of compound tincture of benzoin for such a purpose, for which, however, it is well adapted, and we have used it with marked success.

The use of a bandage beneath the splints for the purpose of controlling the muscles and protecting the skin from the pressure of the splints, is very properly reprobated as useless and dangerous except in certain rare instances. In inexperienced or careless hands this bandage is a most dangerous weapon, and we have had in our own experience to deplore its use, having had the subsequent charge of two cases where this part of the dressing had been unskilfully applied.

We are sorry to say that in common with most American surgeons Dr. Hamilton seems to have a very vague idea of, and therefore does great injustice to, Mr. Lister's method in the treatment of compound fractures. Did space permit, we think that the above charges could be readily substantiated, but we cannot refrain from expressing surprise at the partial success of the method being attributed to the continuous application of a mild, stimulating lotion, etc., when those who have used carbolic acid must know that in a short time it renders the granulations flabby and pale, actually arresting cicatrization, and that the whole aim of the "protective" is to prevent carbolic acid reaching the wound, as it would infallibly *produce suppuration* and retard cicatrization. Having used Lister's method extensively, *as well as other kinds of dressing*, we speak from actual experience and observation.

In treating compound fractures the real essential is "that no additional injury should be done the limb by rude handling, by forcible extraction of slightly detached fragments," etc., Dr. Hamilton says, yet two pages back he recommends their removal "if they are very easily moved about with the finger." We do not wish to be captious in our criticisms, but having already expressed our dissent from such removal of fragments, we like to quote in our own defence such an authority as Dr. Hamilton.

We must also positively disagree with Dr. Hamilton when he says that "there are no circumstances known to me when, according to my later experience, it would be proper to apply ice or cold dressings in compound fractures, unless to restrain hem-

orrhage." Having frequently done so in the past, with benefit, we do not feel inclined to change our practice, especially as Dr. Hamilton does not support his views, by stating his reasons against the use of cold.

In referring to Dr. Barton's bran-dressing for compound fractures, he evinces his usual good judgment in recommending it highly. The chapter on delayed union presents nothing new of importance, except a reference to Dr. Muhlenberg's tables found in Dr. Agnew's work. We think that both these gentlemen rather underrate the frequency of this sequel, since many cases never are put upon record. When speaking of partial fractures, Dr. Hamilton notes the fact which we ourselves observed nearly ten years back, that in the course of time bones so injured are undoubtedly straightened out by either their own inherent elasticity, or by muscular action, or by both. Dr. Lewis D. Mason's plan of treating broken ossa nasi by passing a hare-lip pin beneath the fragments, thus getting a firm basis for support, the author thinks likely to be useful in certain cases, but needing the test of experience. Dr. Mason has since tried it very successfully in two cases, and we have also demonstrated its usefulness in a bad compound comminuted fracture of the nasal bones.

We should like to call the attention of Dr. Hamilton to a case which we reported a number of years ago, where a rib was broken by muscular violence in an exceptionally healthy man, so that, in the next edition, his readers will not inevitably gather the erroneous impression that only a diseased rib can be broken by muscular contraction. In treating fractures of the ribs, we cannot agree with him that adhesive plaster is a poor dressing which is apt to slip. Properly applied, it is the reverse, and by chiefly limiting the movements of one side of the chest, is far preferable to the plan recommended by our author. In like manner, Dr. Sayre's dressing for fractured clavicle, or a modification of it that we have used for six or seven years, we have found to be, on the whole, the most comfortable and efficient, and we must, therefore, take exception to Dr. Hamilton's condemnation of it.

The occasional occurrence of gangrene after fracture through the base of the condyles of the humerus, is well illustrated by several cases given in detail, in several of which suits for malpractice were instituted. If the custom of very early removal and redressing were the rule followed, as advised by our author, no doubt these accidents, although sometimes rendered inevitable by the original accident, would be much less frequent. His advice

with regard to fracture at the base of the condyles, complicated with separation of these processes and consequent implication of the joint, is too often overlooked or unknown, viz., to pay but little heed, comparatively, to the fracture for the first few days, but to devote our main attention to the subduing of the inflammation. Of course, by this is not meant total neglect of the position of the fragments, but an early reduction with *moderate* attempts to retain them by splints and bandages. More active retentive measures we have seen result in a necessity for the total removal of the dressings. Having tried this plan for at least ten years, and never having had cause to regret it, we can only endorse in the fullest manner the author's views. In this fracture, as well as in that of either of the condyles separately, he strenuously urges the importance of a *daily* removal of the dressings, with passive motion. He also advocates the permanent removal of the splint at a very early date to avoid anchylosis—as early as seven days in the “majority” of cases, when, although no union has taken place, “yet the effusions have somewhat steadied the fragments, and the danger of displacement is lessened, while the prevention of anchylosis demands very early and continued motion.” He has entirely overlooked the admirable papers of Dr. O. H. Allis, of Philadelphia, in this connection, who clearly demonstrates that both internal and external angular splints *must* produce deformity by tending to make the long axes of the arm and forearm to coincide, which they normally do not. He maintains that the straight position, with an “apparatus immobile” which admits of the different segments of the upper limb being placed and maintained in their proper relations, is best for the first few days. He also shows that in this position the strong intermuscular aponeurotic septa attached to the condyloid ridges, act as splints to retain the fragments in line. We are glad to see that Dr. Hamilton devotes some space to demonstrating the importance of treating fractures of the radius above the insertion of the pronator radii teres in the supine position, and suggests that where there is a difficulty in maintaining this supination, we should place the whole extremity in an extended position upon a pillow, when this difficulty will disappear.

We can add another case to the two mentioned by Dr. Hamilton, where a fall upon the *back* of the hand produced a Colles' fracture. In such injuries emphasis is laid upon the paramount importance of subduing the inflammation, and the early resort to passive motion, or even entire removal of the splints, if they seem to increase the inflammation by their pressure, etc. We think,

however, that Dr. Hamilton's advice, although judicious in the main, falls short of what we have a right to expect, in that he fails to point out the true indication in the treatment of Colles' fracture, viz., that the radius is a curved bone with its concavity directed toward the flexor side of the forearm, and that it is, therefore, impossible to treat it with a plane splint. By the author's dressing, the padding of the splint does away with this difficulty. In Bond's, the compresses serve the same purpose. In Gordon's apparatus, as well as in the simpler modifications of it which have been introduced, we have a splint approaching in shape the form of the bone. We are as much opposed as Dr. Hamilton to the manufactured splints reprobated by him, but a splint which will correspond to the shape of both radius and ulna, can be made of a piece of shingle and four inches of broomstick in a few minutes, by the aid of a penknife and a few tacks. Space will not permit a description of this splint, which is doubtless familiar to our readers, but several accounts have been published of it by those who claim it as their invention. The author still continues skeptical as to the occurrence of fracture of the coronoid process. We would point out to him that he is in error in his anatomy as to the flexed position not affording more security in such supposed injuries, for in this position the head of the radius completely articulates with the capitellum of the humerus, this process *facing forward so as not to be seen in a back view of the bone*, while in the extended position the articular surfaces are *not usually in actual* contact when the muscles are quiescent, and when they do contract, the amount of articular surface in contact is not great.

We are surprised to find omitted Bryant's line in the diagnostic points of fracture of the neck of the thigh bone. When the bony points can be made out—and they usually can—we have found it of the utmost value. We would say as to the symptoms of epiphyseal separation of the head of the femur, which we have “no means of determining,” that, should Dr. Hamilton issue another edition of his work, we, having met with such a case, will be happy to supply the details as far as possible. The condemnation of plaster of Paris for recent thigh fractures we regard as eminently proper. The analysis of cases so treated certainly shows a number of serious accidents and bad results out of all proportion to the number of cases treated. We agree with the author and Dr. Hodgen, that it is an anatomical impossibility to secure *permanent* points for extension and counter-extension. In case of a compound fracture with secondary hemorrhage, as the author points

out, it would be impossible to reach the bleeding vessel rapidly, or effectually apply temporary compression above the wound. Such an accident has occurred. This dressing has not, Dr. Hamilton thinks, been used for at least two years at Bellevue, for *recent cases*, as far as his knowledge goes. We are not convinced of the necessity of a long splint extending as high as the axilla in fractures of the shaft, but consider that one reaching well above the crest of the ilium is sufficient, with a shorter one inside, both being, of course, appropriately padded. Occasionally it may be found necessary to use short splints surrounding the point of fracture, when there is much bowing of the fragments. In the section on fractures near the base of the condyles, there is serious error in proof-reading, which makes the author contradict himself, viz.: "If the direction of the fracture is from before, *upwards* and backwards, as happens only very rarely, there is danger of the fragments pressing upon the popliteal artery, vein, and nerves, and causing a secondary hemorrhage, or gangrene of the leg." * * * This sentence should read from "*above, downward and backward.*"

We trust, for Dr. Hamilton's own comfort, that he may never have a fractured femur with the adhesive straps for extension applied as he recommends, since the strain on the knee joint soon becomes excessively painful. All this can be readily obviated by extending the straps and bandage *above* the knee; of course, taking great care to stop short of the fracture. This is a practical point which we have observed in our practice.

The chapter on fractures of the patella has been rewritten, and is in substance the work upon that subject which we had the pleasure of reviewing but lately in the columns of this journal. We need only say that it deserves the highest commendation, but that the reader will have to refer to our review,* or the original for details. With all due deference to Dr. Hamilton, we must emphatically dissent from the doctrine that the ankle, in its normal state, "approaches most nearly a ball-and-socket joint of all the ginglymoid joints." What it may do, when the malleolar arch is injured we cannot predicate, but we are convinced that nothing but flexion and extension occur here normally, and that the other apparent movements take place in the calcaneo-astragaloid and astragalo-scaphoid joints.

That boxes are rarely useful, except in certain compound fractures, is, we think, unjust to this time-honored method. With proper care we consider it the simplest and safest plan. We do

* ARCHIVES OF MEDICINE, vol. iv, p. 202, 1880.

not think it so advantageous for the patient to move freely in bed, so that the box being weighty and cumbrous, need not prevent its use. By interdicting sitting up in bed till union takes place, and by pads, etc., obviating the tendency for the heel to fall back, the best results can be obtained. As soon as union commences, a plaster dressing may then be substituted.

The article upon gunshot fractures presents nothing so unusual as to warrant special notice ; it is good, and the teaching is sound and to the point.

Next in order comes the portion of the book devoted to the consideration of dislocations. We wish our readers to understand at the outset, that we consider this portion of the work most excellent, and that those portions which are not criticised receive, in the main, our hearty endorsement. Both in this section and that on fractures, there are very many interesting points that we wish we had space to consider. The emphatic words by which he characterizes the qualities that should be possessed by one who attempts to treat luxations, should be well studied and digested by all surgeons, old as well as young. As to the points which we demur to, the first is, we consider, a clear anatomical error, and evinces a want of the anatomical knowledge which the author has been just describing as a *sine qua non*, viz., that the temporo-maxillary joint has properly but one ligament. It has a very marked posterior and internal portion of the capsule, as well as the long internal lateral ligament which we cannot agree with Dr. Hamilton is unimportant in considering the displacement of the lower jaw.

We are glad to see that Dr. H. H. Smith's method of manipulation in reducing luxations of the humerus, is given a prominent place in this edition. Having frequently used it with success, we are surprised that it is not more of a favorite. Dr. Hamilton has certainly not revised the section on dislocations of the lower end of the ulna, as the identical words are used in its opening sentence which appear in the fourth edition. Now we reported one case ourselves in the *Philadelphia Medical Times* at least six years back.

In the section on luxations or rather subluxations of the carpal bones among themselves, we hope in the next edition to see mentioned one of the scaphoid *forward* reported by us too late for the present issue.

The section devoted to luxations of the femur is, in most respects, admirable, but we think that he exaggerates the risk of

manipulation in their reduction *in skilful hands*. As he himself has insisted upon this skill and anatomical knowledge, we wonder why the blame for injury is laid upon the method and not upon the operator. But few modern surgeons are *practical* anatomists, and still more rare is it to find one who is enough of a practical mechanic to appreciate the tremendous leverage he is using when manipulating the thigh. If any surgeon will make himself familiar by dissection with the hip-joint, and study up his mechanics a little—just a very little,—we think that cases of injury by manipulation will be more rarely reported. We think more extensive quotations from Dr. Bigelow's work would have been advantageous as aiding in deciding what cases are proper for reduction by manipulation, and those where extension, etc., is more appropriate. The author justly points out that neither method will avail in every case. We are indebted to him, however, for emphasizing the fact that manipulation is not a perfectly harmless procedure, and that we have no right "just to try it without ether first," as is often said, under the impression that no harm is possible. No attempt but a well-considered one, which is continued or supplemented by other means until success is attained, ought ever to be undertaken.

In dislocations of the lower end of the tibia, as in Pott's fracture, the author recommends plaster of Paris dressing watched most jealously.

The chapter on compound dislocations of the long bones is specially good. Having pointed out that the tension of the muscles is the chief source of danger in these cases, and that this danger would be present, in a degree, in all cases of fracture were it not for the shortening, which is thus actually conservative, he considers in detail the various plans of treatment, and advises most strongly against reduction without resection, although some cases occasionally do well under such a plan. It is impossible to give anything like a complete idea of Dr. Hamilton's sound teachings on this subject in a condensed form, so we must refer those desirous of more extended information to the original.

As the chapters on congenital dislocations are merely introduced to render the work more complete, and as our allotted space is nearly consumed, nothing special need be said of this portion of the work, which, as far as our knowledge goes, is good and up to the times.

In conclusion, we would express the hope that this work will, in the future, really be thoroughly revised instead of partly rewrit-

ten. It will take but little trouble to make it, in all respects, the best in any language upon the subjects treated of. In many respects it is this now, but, as we have partially pointed out, there are too many omissions—omissions more frequent than we have mentioned—which mar its usefulness.

The young practitioner comes to this book with the rightful expectation of finding it a reliable guide as to the rarity of a given accident. Too often, owing to carelessness in revision, he finds it, as we have done, only in appearance reliable, since time soon shows him, in current medical literature, many more cases like his than Dr. Hamilton mentions. He waits, however, for the next edition, but, to his disappointment, the new edition differs so slightly from the old that it is hard to tell which is the newer. Surely we have a right to expect that each new edition shall be brought up to the times, at least as far as the relative frequency of the rarer injuries go, by the aid of published cases. If Dr. Hamilton cannot spare the time, some of his less busy juniors would doubtless gladly do the more laborious work of collecting the materials for him. We trust that our strictures will not be misunderstood. They are made in all good faith, are the honest expression of the opinions that we have held for years, and are chiefly an expression of our regret that so valuable a work should be marred for the want of the few more hours of labor which it needs bestowed upon it.

It is a book that no practitioner of surgery, or, indeed, most general practitioners, can afford to be without, and we heartily recommend it to our readers as a reliable guide in practice. Dr. Hamilton, with all its shortcomings, may feel justly proud of having given birth to so valuable an addition to our surgical classics.

(C. B. N.)

ORIGINAL OBSERVATIONS.

CASE OF CLONIC FACIAL SPASM SUCCESSFULLY TREATED BY STRETCHING THE SEVENTH NERVE.

By JAMES J. PUTNAM, M.D., BOSTON.

The patient was a young man of about 25 years, and in excellent health; the symptoms were of the usual type, but not specially severe, and had been present for three years, with one intermission of three months' duration. No cause could be assigned either for the original onset of the affection, nor for its disappearance or subsequent reappearance, and no reflex source of irritation could be found.

Encouraged by the favorable results obtained by Baum* and by Schüssler,† in like cases, with the patient's consent I decided to try the effects of stretching the facial nerve, and the operation was performed on April 24th, with the valuable assistance of Dr. A. T. Cabot, essentially in the manner recommended by Baum.

After a search of some length, the nerve was found and drawn on a blunt hook to the surface of the wound. At this stage in the proceedings, the lack of accurate indications as to the amount of force to be safely expended in pulling on the nerve was strongly felt, and my doubt would have been still greater had I already seen the report of Eulenburg's case which appeared at this very period in the *Centralbl. für Nervenheilkunde*, etc., April, 1880, No. 7. In this case the disorganization of the nerve was so complete that the term "stretching" gives an inadequate and misleading idea of the operation.

In the description of Schüssler's operation, this expression is used, that after three or four pullings the nerve "lay forming a

* *Berl. Kl. Wochenschr.*, 1878, No. 40.

† *Berl. Kl. Wochenschr.*, 1879, No. 46.

small loop in the cavity of the wound," yet the resulting paralysis, though for a time complete, began to improve after a few weeks. I determined, therefore, to loosen the nerve well up, while keeping fairly inside of this mark, and this was accomplished by what seemed a very moderate degree of effort, in two or three successive stretchings. The nerve was carefully scrutinized for signs of congestion, etc., such as were described by Baum, but none were present. There was a good deal of venous oozing during the operation, but no vessels were cut that needed tying. The wound healed perfectly by first intention, though for some days there was enough swelling behind the angle of the jaw to make mastication of solid food impossible.

From the time when the effects of the ether had passed away (not during etherization), it had been seen that there was complete facial paralysis, but the first electrical examination after the operation was not made until the fourth day.

At that time the reaction of the nerve to the faradic current was found already markedly diminished, and in the course of a few days more it had disappeared, and in its place the degenerative reaction of the muscles to the galvanic current developed fully and rapidly.

The later history of the case presents little of special interest. For two months no sign whatever of improvement was seen, unless that possibly the lower lid moved very slightly when attempts were made to close the eye. No disturbance of taste or hearing could be discovered.

When the movements began at last to reëstablish themselves, however, in the month of June, two months after the operation, improvement went on more rapidly than could have been anticipated, so that by the end of another month there was but little to be seen of the paralysis, while the twitching had not returned.

So late as January 8, 1881, nearly nine months from the time of the operation, the patient reported by note: "I have had no return of the twitching in my face, and, as far as I am able to judge, the motion is perfectly natural."

These statements were confirmed by personal examination on January 13th, except that, possibly, the naso-labial fold was slightly less marked than normal. The reaction of the nerve, however, as so often happens, was still notably deficient, while all trace of the degenerative galvanic reaction had disappeared.

The only commentary upon the operation of nerve-stretching which suggests itself to me in connection with this case, is that

which has already been referred to by Eulenburg in the interesting paper above alluded to, namely, that the freedom with which such delicate nerves as the facial can be pulled upon with impunity has been overstated in the reports hitherto published. Certainly the accounts given by Baum and Schüssler did not at all prepare me to expect total destruction of the nerve as the result of the force which we expended upon it, nor is it just to compare, without qualification, the effects of such destruction with those of "nerve-stretching," as that is ordinarily understood, though as regarded from the physiological standpoint, the two may be closely related to each other.

Without undertaking to review the theories and observations with regard to the physiological action of the operation, I may be permitted to say that there seems reason to believe it to be compounded of at least four elements, which may be present in varying proportions :*

1. An inhibitory influence exerted on the central nervous system (predominant in the successful cases of nerve-stretching for locomotor ataxia, reported by Langenbuch, Debove, and others).

2. Reduction of the conducting power of the nerve itself, due directly to the stretching, and securing for the nerve and for the nerve centres connected with it a period of relative physiological repose.

3. A greater or less disorganization of the nerve.

4. An alteration in the nutrition of the nerve by rupture of some of its lymph and blood-vessels, and modification of the walls of others.

Which of these influences, or what others, if this list is not complete, play the active part in such cases as this before us, cannot be finally decided till we know more of the pathology, or pathologies, of the affection itself.

Evidently the presence of irritation of the nerve at its exit from the skull, as invoked by Baum as the active cause in his case, is not always to be made out.

However this may be, it is plainly important, from the clinical point of view, that we should know to how much mechanical violence we can expose such a nerve as the facial without fear of serious results, and as a help toward the determination of this point, I made an experiment in the physiological laboratory of the Harvard Medical College, which deserves a moment's attention.

* *Vide the Boston Med. and Surg. Jour.*, for October, 1880. Report on progress in the pathology of the nervous system.

I exposed, namely, under ether, the facial nerve of a large dog, and found it just entering the parotid, and plainly separated into two branches, each about as large as the whole human nerve, one running to the muscles of the ear, the other to those of the face. Raising first the aural branch on a blunt hook connected with a spring balance, I pulled on it till it parted, which it did under a pressure of nearly 40 pounds, leaving on the peripheral side, fragments of nerve fibre of unequal length, nearly or quite free of connective tissue, drawn out from the parotid gland, as when one pulls in pieces a bit of tow or flax.

The facial branch was then hooked up in similar manner, and steadily drawn upon, while the dog, meantime, was allowed to come out of the ether sufficiently to admit of the exciting of corneal reflexes.

When the tension had reached seven pounds, the motion of the lid was seen to be impaired, and it disappeared at eight pounds, reappearing soon after the pressure was removed.

After a second stretching under the same pressure (eight pounds), however, both reflex and voluntary winking seemed to have been completely lost, certainly very greatly impaired, and as the effect of the ether passed still further away, the presence of complete facial palsy was confirmed.

The dog was next seen on the second day after the operation, and then the paralysis, though marked, had manifestly begun to mend, except as regards the muscles of the ear, whose nerve had been severed. A day or two later, the animal was killed for other reasons.

This experiment, viewed in the light of my operative experience, about the outcome of which I was at that time feeling quite uneasy, impressed two or three points upon my mind.

1. That a knowledge of the exact amount of force brought to bear on the nerve is capable of being made of great service, and that, therefore, the method of steady pulling, which admits of the use of a gauge, is to be preferred to the method of pinching and pulling together, as recommended by Baum.

2. That the patient should be allowed to come partially out of ether, so that the immediate effects of the stretching may be witnessed.

3. That if this last cannot be done, the force used for the first pull should not exceed seven pounds, and that for the second probably not six pounds, unless, indeed, the symptoms for which the patient seeks relief are so severe that the production of a

paralysis of months' duration may be fairly risked for the chance of getting rid of them.

Of course these numbers have no exact significance, especially as the proportion of connective tissue in a dog's nerve may differ somewhat from that in the human nerve. Further experiments are called for to settle this and other points.

A CASE OF ABSCESS OF THE LEFT FRONTAL LOBE OF THE CEREBRUM, WITH SPECIAL REFERENCE TO LOCALIZATION.

By E. C. SEGUIN, M.D.

On April 11, 1880, I was asked by Dr. J. Lewis Smith to see a case in consultation with himself and Dr. J. R. Leaming. The patient was a young married woman, aged about 28 years, who had formerly enjoyed good health and had borne several children. During the month of February one of these children had died after a severe illness, and she had undergone considerable fatigue. She seemed depressed, weak, and anæmic afterward.

About four weeks before the date of the consultation she complained of pain over the left eye. This was soon accompanied by swelling and exophthalmus, and on March 24th Dr. Knapp was called in and diagnosticated orbital (sub-periosteal) abscess. This was opened on March 26th by Dr. Knapp.

It was remarked that the pus was under great tension, and that it spurted out a considerable distance when released. Pain ceased at once, the exophthalmus disappeared, and the wound quickly healed. During the first few days of April all seemed going on well; the wound was healed; the patient was free from pain; she was taking tonics, and on the 3d made a call on a near neighbor.

During the night of April 3d and 4th, one week before my examination, she awoke with severe headache and vomiting; ever since she has lain abed, presenting the following symptoms: headache, chiefly mastoid and through the base of the skull; occasional vomiting; irregular respiration; irregular and very slow pulse, varying from 60 to 50 beats per minute; stupor and general feebleness. As negative points there were no symptoms about the eyes, objective or subjective, except a partial ptosis of the left upper lid (which had been incised); no fever, chills, convulsions, paralysis, aphasia; at no time had there been coma. The urine was free from albumen.

Examination.—Patient was soporose, but could be roused by loud speaking; she answered questions as if half asleep, but in such a way as to leave no doubt as to the preservation of language. She put up both hands to the mastoid regions when indicating the seat of pain. A minute inspection showed no paralysis except about the left eye, whose upper lid drooped and whose internal rectus was inert. The pupil on the left side was not fully dilated, but it was a little wider than the right. The optic nerves appeared somewhat congested, and were dim at their periphery, but there was no actual choking. Patient appeared to feel pinching well everywhere. The thermometer showed no fever. The pulse varied from 53 to 66 beats per minute, and it was a reluctant,



Fig. 1.—Apparent location of the abscess, drawn on an Ecker's diagram of the brain.

delusively full pulse, with no real strength. The breathing was easy and regular, but friends of the patient described quite well a Cheyne-Stokes breathing which they had observed. There was neither redness nor tenderness about the site of the orbital abscess.

I diagnosticated an abscess of the brain probably in the left frontal lobe, and expressed the opinion that the patient was in imminent danger. She died the next day in a comatose state; no new symptom having been observed.

It was then learned that for two years Mrs. F. had suffered from frequent attacks of headache, lasting several hours. The pain

was frontal, and sometimes extended along the nose and into the left temple. There had never been symptoms of chronic nasal catarrh.

The autopsy was made on April 13th, about thirty hours *post mortem*, in the presence of Drs. H. Knapp, J. R. Leaming, J. Lewis Smith (the attending physician) and Richard Wiener. We found a large abscess the size of an English walnut in the left frontal lobe. It seemed to lie wholly under the cortex cerebri, in the convolutions of the orbital lobule and in the second frontal convolution. Viewing the hemisphere from the side, the apparent posterior limit of the abscess was the anterior border of the lower part of the third frontal gyrus. Fig. 1. indicates the seat of the soft, fluctuating, bulging abscess. Its depth and penetration were not then determined, as it was thought best to harden the brain as a whole, before making sections.

The external connections and origin of the abscess were most interesting. There was only one point of adherence between the diseased frontal lobe and the dura mater, and that was over the orbital plate of the frontal bone immediately under the swollen frontal lobe. There the dura mater was thickened and adherent to the pia mater and cortex cerebri, forming the inferior wall of the abscess, over a space as large as a ten-cent piece (about 15 mm.). Under this patch of pachymeningitis the orbital plate of the frontal bone was necrosed and perforated; a probe was easily passed into the orbit.

In the orbit, under its periosteum, pus was found, and a part of the roof and the inner wall of the orbit were carious. Careful dissection by Dr. H. Knapp showed disease of a similar kind in the ethmoidal cells and frontal sinus. I need say nothing more of the conditions of these parts and of the pathology of the orbital abscess, as the case has been fully reported from this point of view by Dr. Knapp.*

The appearance of the necrosed orbital plate and of the thickened, adherent dura mater, was precisely similar to what I have several times seen in cases of suppurative disease of the internal ear with cerebral abscess by contiguity. The genesis of the abscess must have been alike in the two situations.

In December, the brain having been sufficiently hardened in bichromate of potash solution, I imbedded it in Gudden's microtome, and made several horizontal sections through the whole brain with the view of demonstrating the relations of the abscess.

**Archives of Ophthalmology*, vol. ix., p. 185.

These cuts showed that the abscess was of quite as large a size as at first supposed, almost perfectly globular in shape, measuring about 38 mm. in diameter. It contained ordinary pus, and was lined by a distinct membrane 1-2 mm. thick. The anterior, inferior and external limits of the abscess were thinned cortex and pia mater; superiorly, posteriorly, and internally, it was bounded by apparently normal white substance. The whole of the white centre of the frontal lobe, except a portion near the convexity of the hemisphere, was destroyed to within 10 mm. of the folds of the island of Reil, and about 8 mm. of the head of the nucleus cauda-



Fig. 2.—Relations of the abscess as shown in a horizontal section of the brain made at the level of Broca's speech-centre. Drawn from a photograph of the specimen. Occipital lobe cut off.

tus. The mass of white substance connecting the inferior and posterior part of the third frontal convolution and the anterior gyri of the island of Reil with the internal capsule, was uninjured.

This last fact is of capital importance in estimating the bearing of this case upon the current notions of cerebral localization.

The above description of the topography of the lesion, especially its posterior limitation, is made from the surface exposed by the lowest cut made, viz., one passing through the speech-centre of Broca, about 10 mm. above the apparent commencement of the fissure of Sylvius (pia still adherent). Fig. 2. is faithfully drawn from a photograph taken of this section-surface. The rest of the brain was healthy to the naked eye.

This remarkable case seems to me of much importance as a negative contribution to cerebral localization. It is in exact accord with recent experimental data, and with the *post-mortem* finding of the last ten years, that an abscess placed like this one should give rise to no motor symptoms, and should not cause aphasia. It is wholly within what are now called the inexcitable districts of the brain. The only symptoms present were the partial paralysis of the left third nerve (more immediately caused by the orbital abscess?) and signs of intracranial pressure. Yet it is important to note that in spite of the enormous pressure which must have existed there was no actual neuro-retinitis.

I have elsewhere reported another case of (smaller) abscess in precisely the same location (left frontal lobe) in which no symptoms referable to this lesion were present.*

On the other hand numerous autopsies are on record in which a smaller lesion (softening, hemorrhage, etc.), placed a centimetre further back in the left frontal lobe, involving the posterior part of the third frontal gyrus or the band of white substance between it and the nucleus caudatus, has given rise to severe symptoms, hemiplegia or aphasia, singly or combined.

In the paper just quoted I have described such cases.

AN OPERATION FOR THE FORMATION OF "NYMPHÆ" IN
THE MALE AFTER AMPUTATION OF THE PENIS
CLOSE TO THE PUBES.

By JOSEPH W. HOWE, M. D.

Amputation of the penis is rendered necessary in all cases of malignant disease of that organ when other measures have failed to bring about a cure. The operation is a simple one, but is often attended by troublesome sequelæ, which make the life of the patient a burden or bring it to a premature termination.

The operation known as Teale's is the one commonly performed. The penis is held by an assistant, or is tied with a ribbon, in order to prevent the stump from slipping back under the pubes, and also to control the hemorrhage. The redundant skin is drawn forward, and with a single sweep of the knife or by a series of saw-like cuts the penis is amputated. The integument is drawn forward and cut while stretched, so that it will retract behind the

*A contribution to the study of localized cerebral lesions. Transactions of the American Neurological Association, vol. ii, pp. 122-4, N. Y., 1877.

stump and not pucker up and contract near the urethral orifice, and so assist in the production of a stricture. When the arteries have been tied, and the bleeding from the erectile tissue suppressed by pressure, cold or cauterization, the urethra is split through in the median line two-thirds of an inch, and the cut edges stitched to the integument. By this means the patency of the urethral orifice is ensured, and all contraction at the new meatus prevented. If this simple procedure is not adopted, contraction is certain to take place, which will ultimately induce retention of urine, and perhaps destroy life. Some operators cut the urethra on each side for the same purpose.

When it is necessary to remove the penis close to the pubes, Humphrey's operation is often employed. It consists in making a circular cut through the integument, dissecting it up and folding it on itself for half an inch. Then the corpus spongiosum and urethra are cut through at the point where the first incision was made through the integument, and both are dissected from the corpora cavernosa as far back as the folded skin. The corpora cavernosa are then cut through at this point, and the integument unfolded and carried over the stump and stitched there, while the urethra and corpus spongiosum project half an inch beyond. By this spout-like formation it was supposed that the urine could be directed sufficiently far from the orifice to prevent the neighboring parts from being wet. The principal trouble in all amputations close to the pubes is the constant wetting and excoriation of the scrotum and surrounding parts by the urine. The main object has been, and is now, to make an operation which would enable the patient to eject a stream a certain distance from the body, or to furnish him afterward with an apparatus which would effect the same purpose. In all the cases I have seen, with or without funnels, the tissues were excoriated by the spraying and dribbling of the urine. The last case which came under my care, and which suggested the modification in the old operation, was operated on by me at my college clinic in October last. The penis was amputated close to the pubes for extensive epithelioma. The patient was sent to St. Francis' Hospital, and made a good recovery. When the wound was thoroughly healed we tried to prevent the urine from excoriating the scrotum, but failed to accomplish the desired object.

The patient suffered very much from the excoriated and inflamed integument, and also from some puckering from contraction of a portion of the redundant integument around the urethral

orifice, the contraction being aggravated by the inflammatory swelling.

I then concluded to direct the stream of urine downward by making organs that would be the equivalent of the labia minora or "nymphæ" in the female.

The patient was anæsthetized, and the loose integument of the scrotum and stump, including the cicatrix, was dissected from the urethra, and an incision an inch in length made in the median line through the corpus spongiosum and urethral canal. The edges were then stitched to the integument and a carbolized dressing applied. In a few days union was completed, and a very fair imitation of the labia minora was effected. When the patient desired to urinate, he sat down, drew his scrotum back, and made a good stream of water in the same manner that a woman would, and the surrounding parts were kept by the "nymphæ" perfectly free from urine. He remained in the hospital sufficiently long to prove that this simple modification of the old operation was a decided success, and a vast improvement on all mechanical appliances which have for their object the propulsion of urine to a distance that will prevent it from getting on the scrotum or neighboring organs.

FURTHER NOTE CONCERNING ACCESSORY FACIAL FORAMINA.*

By A. H. P. LEUF,

STUDENT OF MEDICINE.

As an addendum to my note in the June number of these ARCHIVES, I am enabled to report that in a skull in the Long Island College Hospital I noticed a short time ago two left accessory supra-orbital foramina. Upon further examination of all the skulls I could obtain at the institution mentioned, the result was as follows :

Total number of skulls examined, 53.

Total number of skulls showing double accessory foramina, 4 or $7\frac{2}{53}$ per cent.

Two of the four were left supra-orbital. One of the other two variations, which were right supra-orbital, consisted of two accessory foramina, whilst the remaining were two accessory notches.

No double accessory infra-orbital foramina were found.

* ARCHIVES OF MEDICINE, vol. iii, p. 340, 1880.

This will insure, perhaps, a still greater degree of care in making a prognosis after neurotomy for facial neuralgia.

Although I had not seen a double mental foramen, I thought it worth while to reëxamine some inferior maxillæ, and five of them displayed these anomalous formations.

Total number of inferior maxillæ examined, 35.

Total number of these showing variations, 5 or 14 $\frac{2}{3}$ per cent.

Two of the five were *right mental*, and two were *left mental*. The other was double on both sides.

These mental foramina, in the four unilateral specimens, were about 3-4 millimetres apart. But in the bilateral specimens they were 2 millimetres apart.

The left accessory mental foramen was 1 centimetre to the outer side of the symphysis menti, the normal foramen being .5 centimetres higher up, and 2 centimetres external to the accessory foramen.

The foramina of the right side were the same as those on the left, except that they were .5 centimetres nearer the symphysis menti.

The accessory foramen was but .25 the size of the normal foramen.

I also noticed that where there occurred an accessory mental foramen, it and its normal fellow together no more than equalled the foramen of the opposite side; and that each, separately, was smaller than the normal one on the other side.

ERRATA.

In the table of comparative reactions on page 310 of Vol. iv, No. 3 (December, 1880), the 6th line of first column should read: Is separated from the chloroform-solution by alkalis. The 6th line of second column should read: Is not separated from the chloroform-solution by alkalis. The two words *by alkalis* being omitted in each case as printed.

ARCHIVES OF MEDICINE.

Original Articles.

PRACTICAL OBSERVATIONS ON THE MANAGEMENT OF MENTAL AND NERVOUS DISEASES.*

By J. S. JEWELL, M. D.

HITHERTO the medical aspect of medicine (by which is meant that which relates to the use of medicinal agents for the alleviation and cure of disease) has attracted the most attention. Almost as a rule, medical men, as we find them, give very little attention to the hygienic side of their art. They have contented themselves too generally with simply visiting patients, making a diagnosis, and giving medicine. Too often no inquiries worthy of the name are made as to hereditary tendencies, as to the qualities of the air or food, nor concerning the multitude of circumstances and influences, of one kind or another, which make up the web of daily life, all with the view of estimating to what extent they have affected the health of the individual, or what share, separately or as a whole, they may have had in the production of disease. This aspect of medicine, I say, has thus far, as a rule, been practically almost ignored by the mass of the profession.

But even now a change begins to be apparent, though a long time must elapse before the hygienic aspect

* A paper read before the West Side Medical Society of Chicago.

of the science and art of medicine attains the place it is surely destined to occupy. In respect to no other class of diseases are careful inquiries into the conditions of health and causes of disease so important as in regard to those of the nervous system. It is the special mechanism of the animal body through which relations with the outer world are established, and it is the special medium through which the so-called "sympathies" of the body are accomplished.

These things being true, it is of the utmost importance in the practical management of nervous and mental diseases, to inquire strictly into the conditions of health and disease.

It is to this aspect of our art, or some of its phases, that I would direct your attention briefly this evening.

The activities of the nervous system, broadly speaking, are of two kinds—that is, the voluntary, or those which are stimulated by the action of the will, and in connection with which, at certain periods, is a distinct sense of fatigue or exhaustion, and, secondly, those activities which are involuntary or reflex, either excited through the peripheral nerves, or from the brain itself, as from its sensitive zones, especially in man, and which latter excitations greatly depend upon the play of the emotions, whether of a pleasurable or unpleasant kind.

Now, in making inquiries as to what are the conditions of health and disease as respects the nervous system, we must have regard to these two classes of activities.

A vast number of nervous disorders will be found to grow out of the wear and tear and exhaustion of the nervous system, or some part of the same, arising hence from a too prolonged or too severe exercise of its voluntary activities, or, in a word, from overwork.

In the study of nervous diseases, with a view to their

proper treatment, it is necessary, therefore, to inquire strictly as to what the patient has been doing. The propriety of such inquiries becomes very manifest in diseases like writer's cramp, or the similar disorder which makes its appearance among type-setters, telegraph operators, etc., or when, in presence of such diseases as spinal locomotor ataxia, which so often can be traced to over-use of the lower members, as in the case of the conductors of railway trains, or, finally, in diseases connected with other kinds of employment in which the individual habitually over-uses and thus practically wears out the spinal apparatus for the coördination of muscular movements.

But not to follow special instances farther, it may be also said that ill results from overwork in voluntary exercise of the nervous system are met with on a still wider scale in the local and general nervous exhaustions passing under the name of neurasthenia.

To show how necessary it is in the successful treatment of such disorders to be aware of their conditions or causes, it is only needful to say that it is a common thing to meet with cases of posterior spinal sclerosis, or even of disease, of an acute or subacute character, of the anterior horns of gray matter in the spinal cord, leading, on the one hand, to ataxias, and, on the other, to pareses, muscular atrophies and the like, in which the individuals under medical advice have from day to day gone on taking long walks with the view, as they were told, of preserving or improving their failing strength, with the invariable result of an aggravation of symptoms, and therefore of the pathological conditions on which they depend. A little consideration would make it perfectly clear, in such cases, that the only way to secure recovery from diseases largely caused by over-use, is to remove the cause; that is, stop the activities. One grand reason why spinal ataxias, and more or

less acute affections of the anterior horns of gray matter in the spinal cord, as well as of other parts of the nervous system, prove intractable, lies in the fact that we fail to perceive the necessity for putting the diseased organ at rest, all the more so when a lack of rest or overwork is the cause of disease.

This may appear to be a trite observation, but practically it is of unspeakable importance, and in hundreds of cases attention to, or neglect of this point, makes all the difference between success and failure, life and death, in the management of diseases of the central nervous system. We must, therefore, carefully inquire as to what the patient has been doing, not only with a view to find out the cause, but, having found it, to find a rational cure.

But not only is the nervous system worn and exhausted to an unhealthy degree by what is done by the patient, but also by what is suffered or endured.

In a majority of the cases of nervous disease produced in the ways now considered, worry quite as much as work is a cause. Worrying in respect to business matters, or on account of something, real or fancied, of an unpleasant character in domestic and social relations, the unfavorable operation on the nervous system of the various depressing emotions, or the undue indulgence of even pleasurable emotions, may become, one and all, prolific sources of nervous disorder. It is of very great importance in the practical management of neuroses to make due inquiries under this head, and, as far as possible, remove the causes or conditions of emotional excitement so as to procure rest for the exhausted organism. Indeed, after all that has been written, I know of nothing which outranks in importance, and concerning which more needs to be said and done, than in this matter of rest, physical and mental, in the treatment of nervous diseases.

Side by side with this, another matter should be mentioned. By this I mean passive exercise. I have no hope of presenting this subject as it seems to me it deserves to be. You will probably all agree with me in the general statement that passive exercise is valuable, and that we ought often to avail ourselves of it. But very few even of the active-inquiring members of the profession, I fear, have learned to attach to it a proper degree of importance.

It is a very common thing for the physician to deliver himself of the fruitless generality that the patient must be rubbed. But it is not a common thing for the physician to deliberately set himself down before his patient to consider in detail what kind of passive exercise, and to what extent, is really needed.

It is my deliberate opinion that many persons must have perished from lack of proper passive exercise, that could otherwise have been saved. It is my opinion that thousands are permitted to remain in prolonged, or even permanent invalidism, who could be redeemed from this condition by appropriate passive exercise. It is this class of patients which falls into the hands of professional rubbers and magnetizers, or into Swedish movement cures, many times to be greatly benefited, or even cured, where the regular physician had failed. Patients are permitted to waste away, from disuse of their bodies, who might be preserved in fair condition by appropriate passive physical exercise, which tires but does not exhaust.

Take, for instance, the case of hemiplegia, or paraplegia, or infantile spinal paralysis, or cerebral and spinal congestions after they have come to be, in a measure, passive. In some of these cases, and more or less in all, the patient is utterly incapacitated from taking regular healthful exercise. As results, we have gradual wasting or loss, through disuse, of the paralysed muscular system; the whole peripheral

circulation becomes enfeebled, the surface pale and moist, the circulation fluctuating as well as feeble, the patient inwardly restless and excitable, and in much difficulty between the inward impulse to activity and the practical impossibility of indulging it. Sleep becomes imperfect, the vigor of the nervous system is gradually lost, all the functions of the body are performed, sooner or later, in a somewhat feeble and irregular manner, simply from want of exercise, and the medical adviser will continue his attentions from month to month, and even from year to year, without being once aroused to the physical ruin into which his patient is falling, and hence without ever once intelligently directing a plain remedy in the case.

Thorough massage or physical exercise of some other kind, faithfully carried out, would not only lead to improved general health, but might even become an important means of cure in local disorders.

In all cases of general neurasthenia and of limited exhaustions, in all cases of paralysis, nervousness, restlessness, this question of passive physical exercise should be met. It is the business of the physician not only to perceive its necessity, but to lay down, in the most particular manner, the kind and amount of such exercise, and the methods for securing it. He should personally see to it that a competent nurse is obtained, and, if need be, manufactured, to give this highly important phase of treatment. It should involve not merely, nor principally, frictions or fretting of the surface, but thorough kneading of the deeper tissues, down to the bone itself, done in the most tactful manner, as to the force and duration of the treatment, also trembling motions or movements, delicate spitting of various parts of the body, and other kinds of motions that can be easily devised so as to thoroughly improve the circulation, superficial and deep, to impart fresh impulses to the play of

nutritive activities in all accessible parts of the body, and to produce that sense of weariness which makes quietude or rest a comparative pleasure, and all done without exhausting the nervous system, because the exercise is done for, rather than by, the patient.

Not only in nervous diseases, but in all other forms of disorder in which patients are confined to their rooms, as in fever, or the convalescence from it, all internal disorders, as those of the abdomen or chest, might the comfort of the patients be very greatly promoted, as well as something contributed toward their cure, by the use of such means.

Let me, then, commend to your attention this whole matter of rest, on the one hand, from active exercise, and, side by side with it always, passive exercise as the best remedy for the wide spreading evils of a neglect of the former.

Before dismissing this phase of my subject, there are two points in respect to the management of nervous patients I desire to mention. One is the matter of reading. Many of these patients being in-door invalids and unable to pursue any active occupation, give themselves to reading. In my experience I have found it necessary to watch this point narrowly. It is quite possible to thoroughly exhaust and derange the nervous system by prolonged reading, especially of books on exciting topics.

I am persuaded that many patients are retarded in recovery or made worse from day to day by over-reading. This is especially the case where asthenopia is present, or where certain visual defects exist, which in the use of the eyes may put a strain upon the visual-nerve apparatus. I am careful, therefore, to regulate this point in behalf of my patients.

The second point respects the social relations and activities of nervous patients. They are frequently unpleasant even between members of the same family. In such cases

a cure is sometimes impossible without removing the patient to more pleasant or congenial surroundings. Very much stress is laid on this subject by Dr. Weir Mitchell, of Philadelphia, but hardly more than I have come to give it myself.

Then, again, nervous invalids are often overrun with visitors. It is not an uncommon thing for them to be thoroughly exhausted day after day, so that all the good effects of well-directed treatment are dissipated by unnecessary and exhausting social demands and activities. Promiscuous calls, many of them fatiguing and uninteresting to the patient, should be strictly under the control of the physician for the patient's good.

I would next invite your attention to the question of diet or nutrition.

In the great majority of nervous affections, especially in all general neuroses, this subject is one of great importance. All will agree that such patients should be well nourished, but scarcely one out of one hundred physicians, judging from my observations among them, gives this subject the minute attention that it requires. It is necessary to learn just what are the dietetic habits of each particular patient; what kind of food they take habitually; whether they eat rapidly or slowly; whether the principal meal comes at a time when they are fatigued or not; whether they drink much at the time of eating; whether the food is well cooked and of good quality; what articles of diet disagree with them, if any, and in what way; whether they use coffee or tea, and, if so, whether to excess; whether they use alcoholics; if they suffer from indigestion, what particular form, etc.

With a minute history of this kind, carefully made in respect to each case, it is the business of the physician to apply himself with the utmost care to the making up of a

diet list, with reference to accepted general principles, on the one hand, and to a patient's peculiarities, if any, on the other.

Let the object be to secure the greatest possible amount of nutritive material in the smallest volume. Let all of the details be carefully laid down by the physician and attended to by the patient, as regards the articles to be eaten, the time, the amount of drink, the intervals by which the meals are separated, etc. By this sort of care, trivial as it may sometimes seem, results can be secured of the most gratifying character, which are habitually missed for want of attention to the subject.

This is a far more important matter, as a rule, than the giving of any medicine.

It will be impossible for me in this paper to lay down the diet list that I have gradually been brought in experience to rely upon, but I will not dismiss from consideration this part of my subject without making a few remarks in regard to the use of coffee and tea as beverages.

I am persuaded that in a moderate way greater harm is being done to persons with acute nervous temperaments by the use and abuse of coffee and tea than by any other agents. Coffee and tea may be regarded as pure nerve stimulants. As I have come to regard them, they are not tonics in any proper sense of the word, but rather stimulants. They excite the nervous activities, and hence hasten the expenditure of nerve substance and nerve force, but do not quicken or stimulate the nutritive activities, upon the proper performance of which the perfection of nerve structure and the acquisition and maintenance of a normal degree of nerve power depend.

Where there is already a lack of nerve power and abnormal acuteness of nerve sensibility, all agents and influences should be avoided which excite needlessly the activities

of the nervous system, for the result is simply nerve exhaustion.

That such is, upon the whole, the action of coffee, and, in perhaps a less degree, of tea, I have not the slightest doubt. Therefore, in all cases where the nervous system is naturally highly sensitive or disposed toward neuralgias, or to irregularities in vaso-motor action, and in the vast majority of cases of neurasthenia, I have found it a matter of great practical importance to forbid the use of coffee and tea, or, if they are used, to insist that the infusions shall be weak.

I would turn, in the next place, to the question of climate. There are many cases of nervous disease which seem to be unaffected in a noticeable manner by climatic changes, but, on the other hand, there are many cases of which the contrary must be said.

For example, I have found it to be highly necessary, in selecting a climate suitable to active and passive congestions of the brain and spinal cord, to observe the following two rules:

In the first place, to select a warm and steady climate; and in the second place, a position where barometrical pressure is habitually low.

If the climate is not warm, but rather cold during a great part of the year, the action of the surrounding cold media upon the surface of the body produces one of its ordinary results—contraction of the cutaneous vessels. This leads to a pallid exterior, a cool surface, more especially of the extremities. If less blood circulates in the surface of the body, all other things being equal, more must circulate in the interior. If there are vascular areas within the body, the blood-vessels within which are weakened and easily give way under an increase of vascular pressure, you will readily see how a more or less passive congestion, say of

the cord or brain, may be habitually made worse under such climatic conditions.

If, on the contrary, the climate is steadily warm, the circulation of blood in the surface of the body is more free, and if vascular pressure is not diminished, yet the blood is more equally distributed, and, as a result, an internal congestion is benefited. It is also easy to see, upon a little reflection, how high barometric pressure implies high atmospheric pressure on the surface of the body, and that, as a consequence, blood is forced into air-tight cavities, like the cranial and spinal, more freely than would be the case if barometric pressure is low. Hence, I do not think of sending my patients with spinal and cerebral congestions into cold climates, or to reside at the sea level, but, on the contrary, where the temperature is mild, and as high as I can get them comfortably above the sea level, and I may truly say the adoption of such advice is followed by excellent results.

On the contrary, where vascular tension is low and the circulation feeble, and there is, practically, anæmia of the brain and cord, my advice is to go where the temperature is comparatively low and the barometer stands highest, that is, at the sea level, and almost with uniformly good results, so far as climate is concerned.

But a patient with spinal or cerebral anæmia, if sent to a warm climate and to a point far above the sea level, is almost uniformly made worse. It is, therefore, as you may be already fully aware, a matter of importance in the management of nervous diseases to study carefully to adapt climate to cases.

Happily, in most instances this can be done on generally recognized principles.

We have all met with instances of weather pains in old cases of rheumatism and in neuralgias, occurring especially

just before storms. These have been treated in an interesting manner by Dr. S. Weir Mitchell, who recites certain facts, but does not attempt an explanation of them. According to my own observations, this depends largely, but not exclusively, on changes in barometric pressure. This implies corresponding changes in vascular pressure, especially in certain parts which are the seats of chronic irritative disease of a low grade. These changes in blood pressure give rise to pain in a way not difficult to explain. But my object this evening is not so much to enter on a full discussion of these topics, as to direct attention to the importance of their study with a view to practical results in the management of nervous and mental diseases.

In the next place, I consider as of high importance the procurement of an abundance of sound sleep in the treatment of nervous diseases. Trite as this observation may be, it is in accord with my experience that but few physicians give the practical weight to this subject which it deserves. Too frequently, none but general inquiries are made. The answers of patients are likely to be misleading. It is necessary to make the most penetrating inquiries, not only as to the character and sufficiency of sleep, but what the condition was for the few weeks or months prior to the onset of nerve disorder. For example, inquiry should be made as to whether the patient sleeps best in the fore part or latter part of the night. If sleep is best in the latter part of the night it may be pretty safely asserted that the sleeplessness is not due to a serious cerebral congestion. If the patient, however, sleeps best in the fore part of the night, and rouses soon after midnight, and is unable to sleep the remainder of the night, or sleeps in a fitful way, then it may be declared with considerable certainty that the sleeplessness is due, in part at least, to a passive cerebral congestion which comes on after assuming

the horizontal posture. In that posture, the effect of gravity in diminishing vascular pressure in the head while one is in the upright position, is lost. Blood flows freely to the head; the weakened blood-vessels of the brain gradually expand until at last a more or less pronounced cerebral congestion, partly passive in character, makes refreshing sleep impossible. Sleeplessness depends upon other circumstances, as, for example, upon cold feet, upon irritation from the alimentary canal, upon the occurrence of some noise which, by inquiry, might be ascertained, and, by suitable effort, removed. It may depend upon a variety of circumstances, which it is the business of the physician to find out, and, as far as possible, remedy or remove.

In the few remarks made on this important branch of our subject, it is impossible to do more than refer in general terms to the necessity for sleep in regaining as well as in preserving nerve health.

Another point of great moment, especially in the treatment of diseases of the brain, is attention to the condition of the region supplied and controlled by the splanchnic nerves. In these later times I have learned to consider this as one of the most important regions to which observation can be directed in such disorders. I cannot now trace the history of experimental and other investigations, beginning with the "Klopfversuch" of Goltz and passing on down to the present. But the results of all observations thus far made, whether experimental or clinical, tend to show that affections of this region, to a greater extent than those of other regions, disturb the circulation of the blood, especially vascular tension. The morbid changes in blood circulation, produced by various affections of this region, are noticed perhaps in the most marked manner within the head. It has been long known that disorders of the liver, of the stomach, that certain disorders of the alimentary

canal lower down, more especially those produced by dyspepsia and obstinate constipation, exert an unfavorable influence upon the head. Headaches, vertigos, feelings of pressure, insomnia, dreams, depression of spirits even to the point of decided melancholia, indisposition for mental work, not to speak of other morbid states, are produced from this general source. Chronic gastro-duodenal catarrhs and the dyspeptic disorders growing out of them, other affections of the stomach or of the liver, such as abscess, catarrhal affections of the colon, obstinate constipation with fœcal accumulations, especially bad if in the descending colon and its sigmoid flexure, inflammatory affections of the peritoneum, and a great variety of irritative disorders in the pelvis, both in the male and female,—all these do or may exert an unfavorable influence upon the circulation within the cranial cavity, and fluctuations in the cerebral circulation, produced in this as in other ways, may lead to a great variety of symptoms.

I consider it important, therefore, in the management of affections of the central nervous system, to fix the most serious attention on the region of the body just referred to. I am persuaded that this subject is not usually regarded as so important as I have found it to be in experience.

The physician should make the most minute inquiries as to the state of the organs which lie within the cavity of the abdomen, and whatever is found abnormal, as far as possible, should be corrected. If I should undertake to describe various cases, illustrating the point now made, which have occurred in my experience, it would require a long paper in which to state and discuss them. At present I can do no more than direct your attention to the subject.

Lastly, in the management of nervous diseases, I wish to speak of the necessity of gaining the entire confidence of the patient.

Nothing is better established than that many diseases of the nervous system, more especially of the brain, affect the operations of the mind in many ways and degrees. It is equally well known that the influence of the mind over the body, more particularly over the nervous system, is potent for good or for ill. Discouragement, fear, forebodings, loss of confidence, all forms of depressing or distressful emotions, affect unfavorably the nervous system. On the contrary, an inward sense of security, peace of mind, hope, and nearly all the pleasing and joyous emotions, at least in moderation, exert a good influence upon the nervous system. I know of nothing in its way much worse in the management of nervous and mental disorders than the states of distress and uncertainty in which patients are so often left by their medical advisers. On account of a want of frankness, lack of thoroughness in study of a case, a neglect of suitable pains to make the situation perfectly clear and satisfactory, patients are left in a distressing state of uncertainty as to the nature and probable result of their disorders day and night, burdened with foreboding and wearing anxieties in regard to the fate in store for them. I know but few conditions more harmful to the nervous and mental invalid. Not only for the sake of the patient, but for the sake of the physician, all possible pains should be taken to put the mind of the former at rest by full statements, by clear explanations, by an entirely frank demeanor. The physician should not rest without having made reasonable endeavors to secure the confidence of the patient belonging to the class now referred to. If the patient is sane, and yet his condition is such as to make this difficult or impossible, then my advice is that the physician should deliberately lay down all responsibility, by ceasing to manage the case. No good, as a rule, can come from trying to hold on to nervous patients under these discouraging circumstances.

SOME POINTS ON STAINING *IN TOTO* AND DRY SECTION CUTTING.

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THERE are so many different methods given in the hand-books for microscopical technique, that it may almost appear superfluous to add a new (at least in some respects new) one, to those already in existence. Still, these do not always prove satisfactory, an account of their requiring too much time, and as my method of staining *in toto* and dry cutting seems to be only known to the few who have seen me make use of it, and who have expressed a desire that I should publish a description of it, I do so, more particularly for those medical men who wish to continue those studies—particularly of pathological microscopy—indispensable to a truly scientific education. It is especially adapted to them, as it enables them to work whenever they find leisure, and to leave off at any stage of the proceedings, without fear of spoiling what is unfinished.

For hardening the tissues—with scarcely an exception—Müller's fluid (containing two and a half parts bichromate of potash, one, to one and a half parts sulphate of soda, and one hundred parts distilled water) is to be preferred, because it hardens the tissues without shrinkage, and almost preserves their natural appearance.

It may not be amiss here to mention that it can be satis-

factorily employed only when the following rules are strictly observed: First, the greater the quantity of fluid—within certain proportions to the size of the specimen to be hardened—the more confidently one may count upon its success.

Secondly, the fresher the specimen the more accurate and reliable will be the definition of the cellular construction of the sections.

Specimens not prepared after this fashion will, under no circumstances, repay the labor and trouble expended upon them.

Besides slightly staining the tissue, Müller's fluid has the advantage of preserving the blood corpuscles in the blood-vessels better than any other hardening agent, but to attain this end, the fluid, which is perfectly transparent, should not be allowed to lose its transparency, and must be very frequently changed—not less than twice on the first day, and then (according to the quantity of the fluid) at least every other day. A small piece—say half a cubic inch—requires about a week to attain the right consistency, when, after first washing it in water, it is transferred into alcohol of about 75 per cent. It is not absolutely necessary to harden the specimen in Müller's fluid first; those put only in alcohol in the beginning will stain just as well. I prefer to use Müller's fluid, because most tissues shrink in alcohol unequally, and thus are apt to spoil the right proportions of the specimen.

Specimens hardened in a two-per-cent. solution of bichromate of ammonia, when proper care is taken that the hardening fluid is changed frequently and the alcohol is perfectly colorless, when put into the staining fluid, will also give very nice results.

To insure perfect success in staining it *in toto* (one of the principal advantages of my method), the alcohol

must be changed as often as it becomes yellow, and the specimen is not ready for staining until the alcohol remains perfectly colorless. The specimen is then removed to the staining fluid, the formula for which I published first in 1878*, which is as follows: Dissolve 5 grammes of best carmine in about 8 grammes of caustic ammonia, then add distilled water until its contents equal 100 cubic centimetres. After equal parts of absolute alcohol and glycerine are added to it, it is put on the hot water bath and kept there until the prevailing odor of the ammonia has almost disappeared, which is usually accomplished in about two hours, and when properly made no sediment will be deposited. The fluid is now ready for use, and has this great advantage over other carmine solutions, that it can be used over and over again, and at the same time acts as a preserving fluid, so that the specimen can be left in it for an *unlimited time*. It never stains too deep a shade, and makes the nuclei come out very sharp. For the examination of glands, where it brings out the lunulæ most perfectly, and also of nervous matter, it can not be surpassed. The disagreeable quality of carmine, to stain well one day and diffuse the next, is thereby entirely obviated.

Another staining fluid with which I have obtained beautiful results, either in combination with the first for double staining, or by itself alone, is that prepared with hæmatoxyline. It is especially to be recommended for pathological specimens, because the hæmatoxyline has a peculiar affinity to the products of inflammation and cells of pathological origin.

I prepare this fluid by mixing Bœhmer's hæmatoxyline*

* Ueber die Zusammensetzung der glandula submaxillaris aus verschiedenen Drüsenformen und deren functionelle structur veränderungen. Würzburg, 1878. F. Staudinger.

* Bœhmer's solution for staining sections is prepared as follows: 1 part crystallized hæmatoxyline is dissolved in 30 parts (weight) of absolute alcohol,

with equal volume parts of absolute alcohol and glycerine. Thus mixed it forms the solution for staining *in toto*. It does not stain so quickly as the carmine solution, and pieces of the afore-mentioned size must remain in it from three to four weeks, when they are taken out and again put into alcohol to harden.

To return to the treatment of specimens stained in the carmine solution alone. They are taken out and, to remove the superfluous carmine, are washed in distilled water for a few hours, then put into weak alcohol to be changed for stronger till no more of the pink coloring matter is drawn from the specimens; then to extract all water from the tissue it is transferred into absolute alcohol, which must be changed several times.

When this end has been accomplished, it is put into spirits of turpentine, also to be renewed twice at least, for large pieces. It is then placed in a saturated solution of paraffin in spirits of turpentine, where it is again left for several days—the time varying with the size of the specimen. Next it is removed and laid on filtering paper, that the turpentine may evaporate; the paraffin remains and fills the crevices, blood- and lymph-vessels, etc. When sufficiently dry, it is embedded in a mixture of paraffin and mutton suet, the proportions of which vary with the seasons. In summer pure paraffin will be found necessary.

The paraffin should always be heated on a water bath, and it will be found very convenient to throw the piece to be embedded into the hot fluid mixture, so that it gets thoroughly soaked, and when cooled off, forms an inseparable mass of equal consistence with the paraffin. I have

1 part chemically pure alum in 30 parts (weight) of distilled water; then so much of the alcoholic hæmatoxyline solution is poured into the alum solution (stirring it all the while with a glass rod) till it changes its color to a dark but transparent violet. It is then left for twenty-four hours in the dark, where a slight sediment will form, which must be filtered off to make the hæmatoxyline fluid ready for use.

never found any tissue, not even embryonical, to spoil by exposing them to a moderate heat, while in the fluid paraffin.

It is then ready to be cut *dry*, and can be preserved in this state any number of years.

For cutting it I employ a sledge microtome, made by Haertel, in Breslau, after designs by Dr. Long, which enables me to make very long sections of equal thickness—from $\frac{1}{100}$ of a millimetre upward. When properly constructed, it is the most perfect instrument of its kind I know, and the cost of it, with two knives and with case is, in Germany, at the makers, seventy-five marks, equal to about nineteen dollars. The section thus cut is transferred to a slide where it frequently has to be unrolled—the only drawback to the method, as it requires some dexterity of manipulation. To dissolve the paraffin remaining around and in the section, a few drops of solution of creosote and turpentine (in the proportion of one to four) are added, and when this has been removed from the glass by wiping, or by drawing it up with filtering paper, it is enclosed in either Damar varnish or Canada balsam.

It happens pretty frequently that the specimen, when in the paraffin, is found not to adhere to it; in that case I throw the whole piece back into the hot paraffin and suet mixture and embed it then anew. By this manipulation I get rid of the superfluous turpentine, which was the cause of my former poor success.

By following this method to the letter, it will be certain to reward any co-laborers in microscopical research, and they will obtain especially fine specimens by staining first in the carmine, and then in the hæmatoxyline solutions. These double-stained specimens enable us to differentiate the individual cells, which take the carmine solution up with equal intensity. As a matter of course, the specimen

stained in carmine has first to be washed out and hardened in alcohol, before subjected to the hæmatoxyline solution. For instance, the neuroglia cells of the spine and brain, cancer cells and other pathological products are brought out with greater clearness.

Another method of staining *in toto*, which was, as far as I remember, first published by Kleinenberg, gives frequently, especially for tumors containing ossificated tissue, very nice results. The specimen is first decalcinated by a concentrated solution of picric acid, which takes several weeks, and then, without being washed out, dropped into an ammoniacal solution of carmine. There it is left for twenty-four hours and then hardened in alcohol. By then subjecting it to the same process as described, it can also be cut dry. For delicate tissues it will be found not to answer very well, as it causes frequently considerable shrinkage.

I obtained the best results with it on brain tissue, nerves, etc., but never equal to those stained with the carmine solution.

It may hardly be necessary to state, in conclusion, that objects thus stained *in toto*, can be preserved in alcohol for an unlimited time, and can also be embedded for cutting in any other manner, according to the fancy of the worker.

A CONTRIBUTION TO THE STUDY OF APHASIA,
WITH SPECIAL REFERENCE TO "WORD-
DEAFNESS" AND "WORD-BLINDNESS."

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SINCE the year 1861, when Broca* awakened a new interest in the study of aphasia by his localization of a "speech centre" in the brain for the faculty of language, numerous attempts have been made to establish a physical basis for the complex psychical phenomena comprised in the term aphasia. Not the least important result thus far obtained from these tentative hypotheses is the now generally accepted conclusion that the problem is much less simple than Broca supposed. That there is no single function of the brain which is strictly a "faculty of language," or any isolated portion of cerebral substance which is exclusively a "speech centre" is now well established, so that for the correct interpretation of this symptom our study must extend over wide districts of physical and psychical function. The following case is recorded not in the expectation that it will throw new light upon this obscure subject, but rather because it confirms the view here expressed. Not only was the cerebral lesion found at the autopsy to be situated elsewhere than in Broca's speech centre, but the symptoms during life demonstrated the im-

* Sur le siège de la faculté du langage articulé. *Bull. de Société Anatomique*, Paris, 1861.

portance of faulty interpretation of word symbols as an element of aphasia.

The main facts of the last illness of the late Dr. Charles M. Allin, of New York, are already well known to his numerous friends in the medical profession, who watched the progress of his disease with painful interest. Throughout his illness he was attended by his partner, Dr. A. Dubois, and myself. He was also seen in consultation by Profs. Austin Flint, Sr., John T. Metcalfe, H. D. Noyes and Dr. Allan McLane Hamilton; and for several months was under the professional care of Prof. E. C. Seguin.

Dr. Allin was born in 1827, and was therefore fifty-two years of age at the time of his aphasic attack. At the age of eleven years he had a long illness, which was called "brain fever." Whatever may have been its real nature the illness was sufficiently severe to seriously endanger life, and for several years retarded his growth. At about the age of thirty years he had an attack of inflammatory rheumatism affecting the larger joints. This was followed by three or four other attacks within the next few years, but none of them lasted longer than from three days to a week, or was attended, so far as we can learn, by any cardiac complication. Twelve years ago he had a well-marked attack of gout, and since then three or four other paroxysms, the most severe one five years ago, after a violent quinzey, when both great toes were affected. For several years before his aphasic attack, he was subject to flatulent dyspepsia, and had occasional outbreaks of eczema. It should be noted here that neither gout nor rheumatism were hereditary in his family, and that the most frequent cause of gout—over-indulgence at the table—was notably absent in his case, as he was usually very abstemious both in eating and drinking. In November, 1877, he had a severe attack of renal colic. The concretion was arrested in the ureter, and was

not discharged until the end of ten days, after repeated paroxysms of colic. The stone, on analysis, was found to be composed of uric acid. On February 1, 1879, he attended a concert in evening dress, and on his way home became thoroughly chilled. During the night he was awakened by pain and oppression in the chest, these symptoms continuing during the following day. As there was no evidence of pulmonary or cardiac lesion, but merely tenderness over the middle portions of the chest anteriorly, on both sides, with pain in these situations on movement of the pectoral muscles, the symptoms were referred to muscular rheumatism. Within a few days he was able to return to business, but was still so far from well that some more serious disturbance was apprehended by his medical attendants.

On February 11th, the date of his aphasic attack, he was in much better spirits. At half-past eight in the evening he was seen in his office writing a letter. A few minutes before ten o'clock he rang his office bell violently, and was found by his servant lying on the lounge talking unintelligibly. I saw him not more than five minutes afterward. He was conscious, but unable to answer questions except by a confused muttering. The face was slightly flushed; pulse soft, easily compressible, about 90 per minute; the first heart sound feeble, and no murmur audible. Incomplete right hemiplegia and right hemi-anæsthesia. Was apparently aware of the nature of his attack as he pointed to his right arm and left frontal region. By gestures he finally succeeded in directing my attention to important cases in his note-book, requiring attention on the following day. At eleven o'clock he was seen by his partner, Dr. Dubois, and with slight assistance walked up two flights of stairs to his bedroom. On the following morning he complained of paroxysms of pain in the left frontal region. This symptom, which usually yielded to local applications

of hot water, annoyed him frequently for several weeks, and recurred at intervals during the whole course of his illness. Repeated examinations of the heart failed to disclose any morbid condition except feeble action and moderate hypertrophy. No albumen or casts in the urine. Absence of fever, except on the evening of the third day, when there was a slight rise of temperature which lasted only a few hours. From this time his physical condition steadily improved, and by the end of six weeks his general health was fairly restored. Beyond slight paresis of motility and sensation on the right side the only marked change was the aphasic condition to be presently described. During the summer and autumn of 1879 his physical condition remained fairly good. The kidneys performed their work well, although it was evident from the occasional appearance of traces of albumen and casts in the urine, and from the enlargement of the left ventricle without valvular murmurs, that the kidneys had probably undergone cirrhotic changes. At no time was any increased arterial tension noticed in the sphygmographic tracings, but this absence was ascribed to muscular degeneration of the cardiac muscle, as feeble action of the heart was a constant symptom throughout his illness.

In March, 1880, he had another attack which was supposed to be due to a small cerebral hemorrhage. At dinner, while talking with a friend, he suddenly turned his head to the right, and began muttering incoherently. With assistance, he immediately left the apartment and walked to his bedroom, muttering all the way with his head turned to the right. At my visit, half an hour later, when his consciousness was fully restored, he said that the attack began with an explosive noise in his head like a pistol shot. Immediately he heard some one talking to him over his right shoulder, and turned to see who was addressing him.

Every word uttered by himself, he said, was mockingly repeated by this imaginary individual, and the mutterings his friends had heard were his indignant protests against the insult. On examination there was found slight paralysis, with numbness and anæsthesia on the *left* side. These symptoms disappeared after a few days, his mental condition remaining without apparent change. Shortly after this attack it was evident that his heart was failing in power. He frequently complained of breathlessness on exertion, and the heart sounds were feeble, with occasional intermittance of beat. Toward the end of May he was seized with what proved to be his final attack. The symptoms were slight fever for several days; oppression in the chest with shortness of breath; slight cough, generally dry but occasionally accompanied by expectoration tinged with blood; and marked tenderness over the region of the heart. At a few examinations a faint aortic obstructive murmur was heard, or rather a soft blowing sound over the base of the heart near the aortic valves with the first sound. Urine nearly normal in amount; specific gravity varying from 1012 to 1018; no albumen and no casts except a few hyaline cylinders found at one examination. These symptoms were hardly sufficient to warrant a positive diagnosis, but they seemed to point to endocarditis with possibly myocarditis, and this view was confirmed, or at least considered plausible, by Prof. J. T. Metcalfe, who saw him in consultation. The urgent symptoms subsided by the end of a week, but he was still much prostrated, and complained of giddiness and mental confusion. On one occasion he exhibited in a marked form the so-called rotatory phenomenon, turning over rapidly to the right, and would have rolled out of bed had he not been prevented. On June 19th, about 3 P.M., he suddenly became totally blind. Dr. Dubois, who saw him shortly afterward found him still partially blind, but grad-

ually regaining his vision. At my visit, two hours later, he was perfectly conscious, with his sight fully restored. Half an hour afterward he fell into a quiet slumber, from which he suddenly awakened at 7 o'clock, exclaimed "Oh!" and died instantly.

To return now to a consideration of the aphasic symptoms which constituted the most striking and interesting feature of his case, a few preliminary remarks on the essential nature of aphasia may be permitted before analyzing the symptoms in detail.

The interchange of thought between members of the human family is carried on by means of various symbols, that is, by signs which stand for the ideas they represent; for example: articulate sounds, written language, gestures, facial expression, mathematical, musical and other signs. In aphasia this symbolic function, or capacity to *interpret* and *express* thought in a symbolic form—the *facultas signatrix* of Kant—is more or less seriously impaired. In some cases the chief difficulty is in the direction of *symbol-expression* (ataxic aphasia), the concept being present, but failing to enunciate itself on account of some lesion in the motor track concerned in the expression of symbols. In other instances the concept is present in the mind, but the appropriate symbol for it is forgotten (amnesic aphasia). In a third class of cases there is also a defect in the capacity for *comprehending* symbols. Certain auditory and visual impressions, especially those of word symbols, fail to recall into consciousness their corresponding concepts, although the capacity for forming such concepts under the influence of other stimuli may still be retained. When concepts can no longer be formed, the lesion involves the fundamental processes of thought, and extends beyond the sphere of simple aphasia. The latter term fails, however, to recognize the impaired capacity to *understand* symbols, and as most cases

of aphasia present some degree of this derangement, Finkelburg* has proposed to substitute the word "asymbolia" as a generic term for all the phenomena of aphasia. Kussmaul† prefers the term *asemia*, suggested by Steinthal, as being still more comprehensive; "symbol" represents an idea behind it, whereas "sign" often represents merely an emotion. In the following description of the aphasic symptoms in Dr. Allin's case, we shall use the word "symbol" in preference to "sign," as there was no difficulty in comprehending or expressing emotions. Our classification is based upon that of Spamer.‡

I.—EXPRESSION OF SYMBOLS.

a. Disturbances of speech. On the morning following the first paralytic seizure, by which time the general shock to the brain had abated, it was evident that the cerebral disturbance was limited chiefly to the *verbal* expression of ideas. His general intelligence was fairly well preserved, and he understood much that was said to him, but there was a marked defect in verbal expression. His principal difficulty was with proper names and common nouns. When a glass of milk was held before him, he said: "That is something to drink," recognizing at once its several attributes, its color, uses, etc., but the word which combined these qualities into a single concrete expression, or symbol, he could not utter, even when the word was repeated to him. He had less difficulty with adjectives, verbs and adverbs, that is, with words of less concrete symbolic character. His vocabulary of proper and common nouns very soon began to increase. Within the first few days we succeeded in teaching him a number of such words by directing his attention to the movements of the lips and tongue in pro

* *Berl. Klin. Wochenschrift*, 1870, Nos. 37, 38.

† *Ziemssen's Cyclopædia of Medicine*. American edition, vol. xiv, p. 609.

‡ C. SPAMER. *Archiv für Psychiatrie*, Bd. vi, p. 526.

nunciation. My own name, being short and easily pronounced, he learned in one day, and rarely afterward forgot it. Long names of individuals, or long words which he rarely had occasion to use, he seldom mastered completely at any period of his illness. During the summer and autumn of 1879, his vocabulary increased so as to include a considerable number of words used in ordinary conversation. With these he generally succeeded in expressing his ideas fairly well, but an attempt to leave the beaten track resulted in mental confusion and inability to proceed with the conversation. In rare instances his conversational powers astonished his friends, and gave him delusive hopes of ultimate recovery. On one occasion he conversed with fluency on various topics for nearly an hour, with a friend who had not met him for several years and was unaware of his illness. His friend noticed no aphasic disturbance during the interview, and was greatly surprised afterward on learning the facts of the case. Such flashes were, however, only intermittent, and it became more and more evident that anything like perfect recovery was hopeless.

In conversation, true *paraphasia*, that is, the substitution of wrong words, was rarely noticed. Almost invariably the word uttered bore some resemblance to the correct one, and differed from it in only some of its letters. Thus the first letters were usually correct. This fact was of great assistance to him in conversation, as it enabled him, when he knew the first letter, to find the correct word in a dictionary or work of reference, *where he at once recognized it as soon as he saw it*, showing that the concept was present in his mind in a latent form, and needed only the right stimulus to recall it into consciousness. His Medical Register was frequently consulted for physicians' names he was unable to pronounce, as he retained, to a marked degree, his interest in news affecting the medical profession.

In the expression of musical and other non-verbal sounds, as in singing, whistling, and imitation of various significant sounds, there was no observable deficiency.

As regards the *alphabet* and *numerals* the same cannot be said. At the outset of his illness he was able to pronounce only a few letters, and could not count above four. With training, however, he in time learned most of the alphabet, but never succeeded in spelling any but short and simple words. Counting he reacquired quite perfectly, and was able to solve simple sums in arithmetic, that is, to express their answers verbally. Even when unable to do this he could often *write* the answers correctly. When both these methods failed him he was frequently able to recognize the correct answer if shown to him in writing. During the latter part of his illness he supervised his business accounts, and rarely failed to notice mistakes in them made by others. This circumstance belongs, however, rather under the head of *symbol-comprehension* than under that of *symbol-expression*.

b. Defects in writing. At the outset of his illness there was complete *agraphia*. When asked to write the word "cat," he took the pencil in his left hand, and drew three perpendicular lines, naming them one, two, three. As we shall see in a later illustration, this substitution of numerals for letters and words was at first very noticeable. He knew the number of letters required for the word "cat," but there was *no attempt at the formation of letter symbols*, although he was perfectly aware that his straight lines were not letters. Under training he gradually learned to form letters with his right hand, and after several months could copy simple sentences correctly, sign his name in his usual clear and elegant handwriting, and even write short sentences of his own composition, but more than this he never succeeded in accomplishing.

c. Gesture language. The capacity for expressing ideas by gestures seemed to be unimpaired. He retained much of his natural vivacity of manner, more in fact than could have been expected in a person of his keen sensibility, when he found himself cut off from the ordinary modes of social intercourse. His gesture language had always been a prominent characteristic, and now became an important aid in the expression of ideas. Names of individuals and objects, which he was unable to remember or to pronounce, he frequently succeeded in recalling to others by gestural description, and this was very noticeable even early in his aphasic attack.

II.—COMPREHENSION OF SYMBOLS.

Before entering upon this branch of our subject it should be noted that the senses of sight and hearing in the present case were perfect, so far as could be determined by the usual tests. With respect to vision, the only exceptions to this statement were a transient attack of total blindness a few hours before death, and occasional attacks of hemiopia. Prof. H. D. Noyes, who made an ophthalmoscopic examination of his eyes in the autumn of 1879, reports that "he found no remarkable change in the optic nerves or retinae. The arteries of the nerves were rather small, and, with this exception, nothing abnormal was noted."

A.—*Comprehension of Auditory Symbols.*

a. Spoken words. Early in his illness on my remarking to him one day, "Dr. Peters called to see you," he replied, "I don't know him." The name was repeated several times, but he failed to recognize it, although it was the name of an intimate friend. The written name was then shown him. "What a fool I am," he exclaimed, "of course I know him." This was the first instance in which my at-

tention was drawn to the fact that certain auditory impressions failed to be converted into concepts, although the conceptive faculty remained intact. Not long afterward he noticed this peculiarity himself, as was shown by his remarking to me, "The words I can't pronounce are the words I can't *hear*." This observation, the general correctness of which was verified by repeated experiments, points to a very interesting peculiarity in his case. The words over which he stumbled in conversation were words which made no intelligible impression on his mind when repeated to him, and, conversely, the words he failed to understand in conversation were words he had great difficulty in pronouncing spontaneously. The concepts represented by these word symbols we were generally able to recall to his consciousness by other means, such as writing, gestures, etc., but even then he was unable to express them, except after a certain amount of training. This "word-deafness," except when it was possible to stimulate the conceptual centres by visual or other impressions, made it extremely difficult to determine how much of his aphasia was due to the *ataxic* and how much to the *amnesic* element.

b. Musical and other sounds. His appreciation of music was fortunately well preserved, and was a source of much pleasure to him. In attending concerts and operas he exhibited his usual good critical taste. The significance of other sounds, such as the tone of a bell, the striking of a clock, etc., was perfectly understood.

B.—*Comprehension of Visual Symbols.*

On the third day of his aphasic attack a scroll of Scripture texts was held before him, and he was asked to read the following sentence: "We love Him because He first loved us. While we were yet sinners Christ died for us." He read aloud as follows: "We he have two three that I

have to have the same. I have two three." The substitution of numerals for words is here again noticed as in a previous illustration. The words "the same" probably refer to the repetition of "love" in the first sentence. He was aware that this rendering of the text was incorrect; in fact he almost always knew when he read aloud incorrectly, and expressed impatience thereat. Later in his illness when he was able to read sufficiently well to gather from the newspapers the main points of news, he remarked to me that there were always words in every long sentence which conveyed no impression to his mind, and that he was compelled to form his idea of the meaning of such a sentence from the other words whose meaning he understood. The significance of many of these uncomprehended words could be conveyed to him in other ways, showing that his failure to recognize the written symbols was not always due to a defect in the conceptual centre, but rather to a lesion in the channel of transmission from the optical centre for word symbols to their ideational centres.

The same difficulty extended at first also to the comprehension of *written numerals and their combinations*, but, as we have already seen, he reacquired, to a certain extent, this capacity under training. *Gesture language* he understood perfectly from the start.

The degree of impairment in intelligence, otherwise than in the comprehension and expression of symbols, it was extremely difficult to determine for reasons already given. His intimate friends were satisfied that there was much less general mental deterioration than those who met him casually would infer. His memory of incidents in his own life, of the past illnesses of his patients and of numerous other details was strictly accurate, so that we could rely upon his statements upon such points in every particular. In business matters he always manifested his usual tact and good

judgment. During the last few months of his life he was a constant attendant at the surgical operations of the New York Hospital, of which he was an attending surgeon, and his criticisms showed that he retained not merely a general interest, but also his special knowledge in surgery. On several occasions he assisted me in minor surgical operations and dressings, with his usual deftness and attention to details. At whist, euchre and all games with which he had been familiar he was as expert as ever. During the winter of 1879-80 he consulted numerous medical works on the subject of aphasia. Since his death I have seen a sheet of paper containing his notes of reference to articles on this subject in English and French works and journals. The titles, dates, etc., are strictly correct, and are written in his usual clear and elegant handwriting. His memory of location was particularly well preserved. He could always turn without hesitation to the right place in books he wished to consult, remembered the houses of friends—that is, their relative positions in the city—and in numerous other ways showed that he perfectly understood the spatial relations of objects. The only exception to this fact was a singular symptom which annoyed him for several months, viz.: a tendency to reverse the natural position of objects which he handled, such as table-knives, spoons, pencils, canes, etc. He immediately recognized his mistake, however, and corrected it, but always spoke of the inclination as irresistible.

As an aid to the interpretation of the aphasic symptoms in the present case, we reproduce, below, Spamer's diagram representing the several tracts between the reception of impressions, the comprehension of these impressions and their expression.

It will be noticed in the above diagram that the tract from *P* to *B* is represented by a straight line, while the tract from *P* to *B* pursues a circuitous route. By this dis-

tion Spamer attempts a rough explanation of the difference observed in most cases of aphasia between the comprehension of *auditory* word symbols and the comprehension

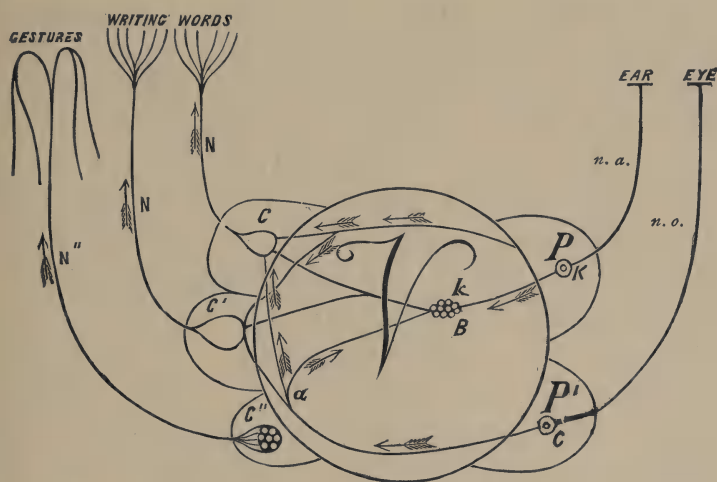


FIG. I.

The circle in the middle of the diagram, *V*, represents the ideational tracts. From the right the excitations of the sensory nerves pass into the brain.

n. a.—auditory nerve. *n. o.*—optic nerve.

P and *P'* represent the places where the auditory (*K*) and the optical (*G*) impressions are perceived. When the impressions reach these points we have merely sense-perceptions without associated conceptions. The association with definite corresponding conceptions takes place only when the excitation travels onward to *B*, the conception. From this point the excitation may proceed to *C*, *C'* and *C''*, the centres of coördination for movements in speech, writing and gestures.

N, *N'* and *N''* are the motor nerves concerned in symbol expression (speech, writing, gestures). At their terminations these nerves are broken up into fibres distributed to individual muscles.

The diagram represents the reception and tracts of *word symbols* through the eye and ear. The tracts of other auditory and ocular impressions are not designated.

of *visual* word symbols. Cases of marked word-deafness without ordinary deafness, seem to be extremely rare; at least there are very few instances of this kind on record. The tracts for *all* auditory impressions, he supposes, lie in close connection, and may be represented by a single straight line. With visual impressions the case is different. Aphasic patients very generally recognize material objects, but exhibit a marked defect in understanding written and

printed words, as well as in expressing the concepts in speech and writing. The tract for visual word symbols is therefore more or less widely separated from the tract for other visual impressions, and lies in some parts of its course near the centres of coördination for speech and writing, or near the tract from *B* to the latter. This explanation is ingenious, but hardly satisfactory. If the tract from *P'* to *B* should be represented by a circuitous route, that from *P* to *B* could scarcely have been *direct* in Dr. Allin's case, because the word-deafness was even more marked than the word-blindness, although both auditory and visual impressions, with the exception of word symbols, were interpreted with equal acuteness. Indeed, our main reliance, when the word symbol failed to be recognized by him in conversation, was to present the word to him in writing. The reverse process, that is, the presentation of the auditory, in place of the visual word symbol rarely succeeded. In other words, he seldom understood the spoken words when he failed to comprehend the written form.

DR. SEGUIN'S REPORT OF THE AUTOPSY.

The autopsy was made twenty hours after death, on June 20th. The body was well preserved in ice.

Head.—The dura mater is abnormally adherent to the calvarium, on both sides equally; no thickening of dura. Pacchionian bodies small. Marked subarachnoid effusion which has gravitated to posterior regions. Dura of base normal. The basilar artery is really a continuation of the right vertebral artery; the left being only 1 mm. thick. The right vertebral and the basilar arteries are the seat of patches of arteritis, separated by regions of healthy tissue, but nowhere obstructing the flow of blood. Circle of Willis is complete and patent. The carotids, just below the circle of Willis, are extraordinarily thickened, quite rigid, but not calcareous; their wall is nearly 1 mm. thick. The same al-

terations in patches can be traced in the accessible branches of the middle cerebral arteries; the anterior cerebrals are only slightly affected. Nerves at the base normal. The left hemisphere is the seat of a large depression caused by the destruction of several convolutions, viz.: the whole of the inferior parietal lobule, with the first tier of temporal gyri. The posterior extremity of the angular gyrus, and the whole of the ascending parietal are preserved. This lesion is a yellow patch lying in the region supplied by the terminal branches of the left middle cerebral artery. To external examination, the remaining convolutions are normal, more especially the third frontal, the ascending frontal, and the anterior gyri of the island of Reil. The first branch of the middle cerebral artery on the left side is pervious, though there are a few patches of arteritis near its origin. The main trunk of the artery, in the fissure of Sylvius, and its two terminal branches are pervious to the confines of the patch, and in the pia covering the patch. The patch was probably caused by blockade of smaller arteries which cannot be traced. The right hemisphere presents a healthy surface. On opening the fissure of Sylvius, the middle cerebral artery is found patent but bearing a few patches of thickening.



FIG. 2.

Lateral view of the left hemisphere of Dr. Allin's brain, showing the superficial extent of the patch of softening.

The brain is sliced in transverse vertical sections.

Section No. 1, about 37 mm. (one and a-half inch) from apex of frontal lobes, presents no lesion.

Section No. 2, at a distance of 25 mm. behind No. 1, passing through the posterior extremity of the third frontal

convolution and cutting off the apex of the temporal lobe, is free from lesion.

Numerous fine slices made in the speech tract in this region (left side) reveal no alterations of structure.



FIG. 3.

Diagram of transverse vertical section through left hemisphere, showing the extension inward of the patch. This view corresponds to section No. 4, described in the text.

- | Section No. 3, 25 mm. further back, showing the lenticular ganglion and the thalamus, no lesion.
- | Section No. 4, made at 25 mm. behind No. 3, passing through the anterior limit of the yellow patch above described, and cutting through the posterior extremity of the thalami. There is no lesion to be seen except the yellow patch in the left hemisphere, and its full extent is well shown; besides destroying the convolutions it extends deep into the white substance of the hemisphere to the roof of the lateral ventricle.

Section No. 5, made at a distance of 25 mm. posterior to No. 4, reveals the penetration of the yellow patch as just described.

Section No 6 shows no lesion.

The brain was afterward finely sliced up without any other lesion being discovered.

Sections made at different levels in the pons varolii and medulla oblongata seem normal.

Cerebellum normal.

The *lungs*. There are old adhesions at the right apex; some fluid (250 cc.) in each cavity, with old adhesions over lower part of upper lobe of left lung anteriorly. The pericardium contains 6 cc. of serum.

The *heart* exhibits various lesions; there is a fibrous patch in the anterior wall of left ventricle lying within 12 mm. of the apex and 12 mm. of septum, extending through the septum. There are atheromatous patches on the mitral and aortic valves (at attached border). At the origin of the coronary arteries the atheroma is thicker, and their lumen is somewhat encroached upon.

There are signs of endocarditis; firm masses of fibrin adherent to inside of the heart about 18 mm. below the aortic valves. Beneath these the endocardium is intact, except at one small point about the size of a No. 3 English sound. At the apex of the left ventricle is another fibrinous clot, as large as a green-pea, which is just breaking down. Left ventricle dilated; right cavities normal.

Left kidney small; capsule not adherent; surface indented by old cicatricial depressions; congested; cortex thin; texture firm. *Right kidney* about 68 mm. long and 37 mm. wide; capsule not adherent; cortex almost gone. Renal artery atheromatous.

Liver and spleen normal.

Stomach and intestines not examined.

REPORT OF MICROSCOPICAL EXAMINATION

BY DR. PEABODY.

Both *kidneys* show the lesions of chronic diffuse nephritis, in the larger, the parenchymatous element predominating, in the smaller, the interstitial. In the large one many straight tubes, especially near the papillæ, are lined by the remains of epithelium, which has been converted in places almost wholly into fat. This appears both in the form of granular matter and in the form of minute drops of oil. The cortex shows many partially atrophied Malpighian bodies with Bowman's capsules very much thickened. The walls of the small arteries are much thickened in both kidneys. In the smaller kidney there is marked development of new connective tissue, especially in the cortex, which has caused narrowing of the calibre of many convoluted tubes and total obliteration of others. Bowman's capsules in many places are immensely thickened, and many Malpighian bodies are converted into solid little balls of connective tissue. The cortex is in many places crowded with young cells (in its intertubular tissue), even in places where the atrophy of the Malpighian bodies is already complete, showing that the inflammatory changes in the connective tissue were actively progressing up to the end, and suggesting the possibility of these changes having progressed with activity from their inception. There are nowhere to be found places where large tracts of kidney tissue have been converted into fully-formed connective tissue, as is usually the case when the whole organ is so much reduced in size. Some of the straight tubes contain fatty matter in the place of epithelium as in the larger kidney.

The renal artery of the small kidney is an object of especial interest. It is much smaller than usual in size, and

its lumen has been encroached upon to a marked degree by what is, in section, apparently a crescentic thickening of the intima. To the naked eye this bears no resemblance to atheroma, having none of the look of fat about it, and being grayish and translucent in appearance like connective tissue. Upon microscopic examination it is found that the wall of the vessel between the endothelium and the membrana fenestrata is thickened in the entire circumference of the vessel. This thickening is not uniform, however, being much greater at one point and gradually shading off toward the opposite side, producing thus what seemed to the naked eye as a crescentic growth from the vessel's wall. This new growth consists of a vast number of spindle cells and young connective-tissue cells embedded in a cloudy intercellular substance, in parts of which connective-tissue fibres can be distinguished.

Upon subsequently examining numerous sections of the kidneys, I find that the same condition exists in many of the arteries of the small kidney. In some of them the calibre is uniformly encroached upon from all sides by a growth exactly similar to this; in others the lumen is narrowed from one side only; in others it is entirely obliterated by the same growth.

In the larger kidney I have not found this change in the vessels.

Heart.—The peculiar light-colored patches found at the autopsy in the ventricular septum and in the anterior wall of the left ventricle, are found to be made up of fully-formed connective tissue containing numerous blood-vessels. This extends into the muscular tissue in such a way as to enclose islands of the latter at many points, and elsewhere so as to make long prolongations in the intercellular spaces. It is the form of atrophy known as fibroid degeneration of the heart.

In regard to the character of the changes in the renal artery there may be difference of opinion. So far as I know, it was first described by Heubner* in 1874 as a syphilitic disease of the cerebral arteries. It has been shown, however, by Friedländer,† to occur in a great variety of conditions and various organs, and is by him believed to be by no means always of syphilitic origin. Baumgarten‡ does not regard it as a diagnostic mark of syphilis in the least, and considers it similar in all respects to the endothelial growth which occurs in arteries after ligature, as described by himself in his monograph on the subject.§

Thoma|| has described a similar degenerative change in the arteries of the kidneys in chronic interstitial nephritis. In fact, I should make my part of this paper too cumbrous were I to cite all the authors who have described this condition.

I have myself met with it in three other cases, of which one was clearly syphilitic, there having been no history of syphilis, nor any indications of it, in the other two. In two of my cases I found the lesion only in the arteries of the brain; in the other it was also obvious in several of the arteries of the abdomen, notably the superior mesenteric and its branches.

CONCLUDING OBSERVATIONS BY DR. SEGUIN.

Dr. Allin's paræsthesiæ and perversions of muscular sense were very curious. He referred his sensations of numbness on the right side to homologous regions in the hand and foot, viz.: the distribution of the ulnar nerve and that of the

* O. HEUBNER. Dieluetische Erkrankung der Hirnarterien, etc. Leipzig, 1874.

† *Centralblatt f. d. Medicinischen Wissenschaften*, 1876.

‡ *Virchow's Archiv*, lxxiii.

§ Organisation des Thrombus, Leipzig, 1877.

|| *Virchow's Archiv*, Bd. lxxi.

musculo-cutaneous in the leg and foot. In the right side, generally, the paræsthesiæ were of drawing up, or tightening, and as if a strong rotatory movement were going on in each limb around its longitudinal axis, the hand in pronation, the foot in inversion. The patient's account of these subjective movements never varied, and he would often illustrate them by moving his hand and forearm in extreme pronation and rotation.

The impairment of muscular sense of which he complained was something which I had never met with before. If he did not use his eyes in prehending objects with his right hand, he would find that he had seized them by the wrong end. He sometimes found himself standing with the head of his cane on the ground and its point in his hand. Frequently, in my presence, he essayed to grasp a pen or pencil with his head turned away, and repeatedly he found himself holding the object by the wrong end, and this after turning it over three or four times to get its outlines.

Yet with these perversions of sensibility there was no common anæsthesia, either to pricking, to cold, or to æsthesiometer points.

Dr. Allin's aphasia was complex, but the striking feature in it, during my six months' observation, was the word-deafness.

He could express himself fairly well in short sentences, and might for a little while carry on a commonplace conversation with a non-expert without betraying his defect; but he frequently failed to find the right word, and often found it only after struggling a good deal.

In attempting to speak he would often, after failing to get the proper noun, use a corresponding verb or employ synonyms, showing that his idea or concept was always correct, but that his vocabulary was faulty. He could copy written or printed characters quite readily, but experienced great difficulty in writing spontaneously.

All the auditory relations of language were much impaired. He used to say that going to church and listening to a sermon was to him all a mixed-up, meaningless jargon, like "drub-arub-drub." He could catch very few words. In ordinary conversation, familiar short sentences were apprehended readily; equally simple sentences, containing other than the most commonplace words, had to be repeated again and again. Reading from a book was jargon to him. Writing from dictation was impossible, and even the alphabet was poorly executed in this way. The sound of the letter *c* seemed the one for which he was most deaf.

Yet his hearing was not impaired (I never tested it carefully), and he understood and appreciated music. While a lecture or sermon was unintelligible, he enjoyed a concert and claimed to appreciate it. He whistled and hummed airs correctly—much better than he spoke.

I often questioned about and tested him for hemiopia, with negative results. Occasionally he had attacks of moving fortification lines in the left fields of vision, but these were evidently phenomena of the migraine type.

The pathology of the case is obscure in many respects.

The arteritis (see Dr. Peabody's description) is not of the senile type, and the patient's statement, that he had never been infected, was positive, and, we believe, perfectly trustworthy. This would, therefore, be one of the best authenticated instances of non-specific endarteritis deformans, leading to obliteration of the calibre of small arteries, ischæmia of a cerebral territory, and softening.*

The location of the lesion is peculiar, and some years ago would have been considered as destructive of the modern theory of aphasia. In view of the experiments of

* It is very much to be regretted that the cerebral arteries and the brain itself were not examined microscopically; but the autopsy was allowed only on condition that the brain be not removed.

Ferrier, Munk* and others, however, it seems clear that the lesion occupied a portion of the brain which is concerned in the reception of sensory impressions from various sources, more especially the eye and ear.

So long as aphasia was looked upon as sometimes a form of motor disorder, a difficulty in the emission of language, and in other cases as dependent upon verbal amnesia, it was impossible to explain its production by a lesion of the parietal or sphenoidal lobes. In the last two or three years the elements of imperfect perception of the written signs and spoken sounds of language—word-blindness and word-deafness respectively—have received some recognition, and these phenomena are perfectly explicable by lesions placed in the sensory or perceptive regions of the cortex and internal capsule.

In such cases the aphasia is indirect, not due to any interference with the channel for the emission of sound-forming impulses, but to a break in the other part of the circuit, viz., the receptive organ.

Brief notes of another case of aphasia of this category are appended ;

Male, aged 54 years, seen in consultation with Drs. Dodge and McClellan, of Brooklyn, January 16, 1881. Indefinite ill-health for past few months. About four weeks ago apparent pain in head, groaned, and had severe epileptiform attack followed by two days' stupor. On recovery from unconsciousness had complete aphasia. In three or four days three convulsions in one day ; paralysis not noticed. Aphasia continued. Has seemed irrational to family and physicians of late. Has taken much bromide of potassium.

Examination.—Slight but distinct right hemiplegia, face and body. Tongue straight. No lesion of optic nerves. No ataxia. Can speak a good many words, but not always to the point. Complete alexia and agraphia. A degree of word-deafness also

* Consult : Ferrier, "The Functions of the Brain," New York, 1876 ; Munk, "Ueber die Functionen der Grosshirnrinde," Berlin, 1881.

exists. Often does not understand what is said, yet mind seems clear. Heart normal. Repeated examinations reveal no signs of contracted kidneys. Temperature and pulse normal. Sensibility not tested. Death took place on 25th, in coma after considerable maniacal excitement. The autopsy, made by Dr. J. C. Shaw, revealed a slight diffused recent meningeal hemorrhage (subarachnoid) over frontal lobes, and an older effusion, a common clot, with extensive lesion of the cerebral tissues, in the white matter under the first and second temporal convolutions of the left side, in a region which receives fibres from the posterior half of the internal capsule, and which corresponds to Ferrier's centres 13 and 14, which in monkeys serve for the reception of images and sounds. This case is an illustration of aphasia with word-deafness (probably word-blindness also) caused by a lesion of the receptive part of the speech apparatus. The slight hemiplegia must have been due to transmitted pressure, as the motor regions of the brain, including the third frontal gyrus, were normal.

In the above case the lesion was in the parieto-sphenoidal region, and, as in Dr. Allin's case, the third frontal convolution and associated motor tract were intact. It is interesting to note, in this connection, that we not rarely meet with cases of partial right hemiplegia with incomplete anæsthesia, homonymous hemiopia, and word-blindness or word-deafness, and varying degrees of aphasia. In such cases the lesion is deeply placed near the thalamus opticus or in its postero-external portion, involving the posterior segment of the internal capsule.

The following is a summary of a remarkable case of this class, still living, in which word-blindness (and alexia) is almost the only aphasic symptom remaining, coincident with homonymous hemiopia and partial right hemiplegia and hemianæsthesia.

Mr. H., aged 62 years, seen January 3, 1881. Is a man of unusual intelligence and culture; master of several living languages. Enjoyed good health; had no premonition, and on May 19, 1880, returned home from business with a numb and queer feeling in right arm. Was not well for three days, and on morning of

May 23d awoke with right hemiplegia and incomplete aphasia. Was semi-comatose later in the day, and remained so for some days. There must have been marked hemianæsthesia, as patient did not know when his right foot and hand lay in or out the bed. He spoke much, but miscalled things. Vision was imperfect to the right of the patient. No serious general symptoms. Improved rapidly as regards power of motion, speech and writing. The only remains of the attack are right paresis with ataxic movements and partial anæsthesia, right lateral hemiopia and alexia. Has learned to write with left hand.

Examination.—Very slight right hemiparesis. Awkwardness and ataxic movements of right hand and foot ; marked tactile and caloric anæsthesia of right hand ; feels pain or a simple touch well, yet he is unconscious of passive movements of fingers and hand when his eyes are closed (so-called loss of muscular sense). Has incomplete homonymous (or lateral) hemiopia : right temporal and left nasal fields darkened. No positive contracture. Heart and kidneys normal.

Speech is a trifle thick, but not aphasic to any extent ; occasionally uses a wrong word ; writes well with left hand, and composes a letter in French, German or English as well as ever ; no omissions. Sees letters and numerals, but can't read except by a laborious process of spelling ; some few short and familiar words he reads at once. Hemiopia does not cause this alexia, as shown by his ability to read any word by spelling it. The blindness is for words, or rather for the images or concepts which words represent. There is no word-deafness, and, beyond some hysteria, mental action is good.

As the complex problem of aphasia now appears, studied in connection with recent discoveries in the physiology of the cerebrum, it may be stated as a probability that aphasia with predominant word-deafness or word-blindness, and hemianæsthesia, cutaneous, muscular or sensorial, is dependent upon a lesion placed behind the fissure of Rolando, in regions which correspond to the sensory cortical centres of the monkey and dog as determined by Ferrier and Munk.

REPORT ON THE USE OF HYOSCYAMIA AS AN HYPNOTIC AND DEPRESSO-MOTOR.*

By E. C. SEGUIN, M.D.

I HAVE been instructed by the Committee on Neurotics to present a summary of the experience of its members with that powerful and seldom-used alkaloid—hyoscyamia. Due credit will be given at the proper place to the gentlemen who have contributed observations.

The report is divided into four parts. 1. A brief and, I must add, an incomplete sketch of the history of our knowledge of the drug, and a summary of the conclusions of the few physicians who have employed it. 2. A relation of our own cases testing the value of hyoscyamia as an hypnotic. 3. A relation of the cases showing the power of hyoscyamia as a depresso-motor or paralyzant. 4. Our provisional conclusions respecting its utility and the best modes of its administration.

I. HISTORICAL.

Hyoscyamia in an impure form was discovered by Peschier, in 1821,† and by Brandes about the same time.

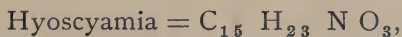
In 1833, Geiger and Hasse‡ found the pure solid alkaloid very much like the one we now use.

* Read before the New York Therapeutical Society, February 13, 1880.

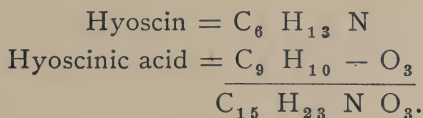
† *Annalen der Chemie und Pharmacie*, i, p. 333.

‡ *Idem*, vi, p. 270.

They determined its molecular constitution to be:



and this formula is accepted as correct even by the latest authorities.* By the same chemists it was shown that H. might be looked upon as a double body, in the same way as atropia, giving a formula:



Hyoscyamia is obtained from the leaves of *hyoscyamus niger* and *h. albus*, in very small quantities; according to Schoonbroodt, about 0.164 per cent., according to Wadgy-mar, 0.143 per cent., and according to Thorey (using the dried leaves picked before flowering), 0.188 and 0.208 per cent.† These experimenters and Merck, of Darmstadt, have greatly improved the processes for extracting the alkaloid.

In our market H. is found in two forms, both of Merck's manufacture:

First, a colored resinous extraction which is quoted at 15 cents per grain (.06).

Second, a white crystallized, or semi-crystallized substance, very pure and exceeding powerful, which costs, at whole-sale, 75 cents per grain (.06).

Both these forms of H. are dispensed by our leading pharmacists, and two firms supply reliable pills made of the crystallized form. Messrs. Caswell, Hazard & Co. offer it in the shape of tablets, each containing $\frac{1}{80}$ grain, or a little more than .001 gm., and Messrs. McKesson & Robbins make little pills, each containing $\frac{1}{80}$ grain. Both of these

* The National Dispensatory, second edition, 1879, p. 747.

† Cited by A. and Theo. Husemann, "Die Pflanzenstoffe," pp. 478-9.

preparations have been found reliable by your committee, the tablets of the first-named firm seeming to be a little stronger.

One of our number, Dr. Ball, has employed an amorphous preparation by Keith; a substance whose physiological properties are not well understood, and which may be a resinoid instead of an alkaloid.

The mode of administration will be stated in connection with the work of each observer.

I have consulted the following writings on Hyoscyamia:

The essay of Oulmont on the use of H. in paralysis agitans, tremors, etc., I have been unable to find, and have not even seen an abstract of it.

John Harley, "The Old Vegetable Neurotics," London, 1869, p. 321.

Dr. Harley's experiments with hyoscyamus and hyoscyamia are replete with interest. He used the tincture, succus, and solid extract of hyoscyamus, and made for himself a brownish, semi-crystallized sulphate of hyoscyamia. He experimented on mice, cats, dogs, and on man. The following are some of his conclusions:

1. Hyoscyamia differs somewhat from atropia, in not accelerating the pulse as much, and by a greater effect on the cerebrum and the motor centres.

2. Small doses will reduce the pulse-rate by 10 or even 30 beats. Larger doses cause a rise.

3. Sleep and great muscular relaxation are produced. To cause sleep, $\frac{3}{4}$ jss of the succus hyoscyami or $\frac{3}{4}$ j of the tincture are required, and from $\frac{1}{30}$ to $\frac{1}{8}$ grain of hyoscyamia (hypodermically) will do the same.

4. In one case 8 grains of the solid extract produced a busy delirium not unlike that caused by hemp. The effects of H. are very like alcoholic intoxication.

5. Its action on the cerebrum is very similar to that of

opium, except that it does not cause excitation of motor centres. No antagonism exists between the two.

6. Clinically, it is useful as a sedative to the heart. It is useless in convulsive affections. (In view of our own experience this last statement seems unaccountable.)

7. H. is excreted through the urine.

A. und Theo. Husemann, "Die Pflanzenstoffe," Berlin, 1881, p. 475. These authors give an excellent pharmaceutical account of the alkaloid, and a rather meagre summary of the physiological researches of Schroff, Lemattre, Buchner, Frömmüller, Düllnberger, etc.

Their conclusion is that the action of hyoscyamia is qualitatively similar to that of atropia. Its chief use is as an hypnotic.

Schroff thought H. produced more complete and permanent dilatation of the pupil than A.

Dr. Robert Lawson, Assistant Medical Officer to the West Riding Asylum, has contributed to the celebrated medical reports of that institution two elaborate papers on hyoscyamia; one, "On the physiological actions of hyoscyamine," appeared in the fifth volume, 1875, and the other, entitled "Hyoscyamine in the treatment of some diseases of the insane," in the sixth volume, 1876.

The following are some of the conclusions reached by Dr. Lawson in his first or physiological study. He used an amorphous hyoscyamia made by Messrs. Smith, of Edinburgh, evidently a weaker preparation than Merck's:

He found hyoscyamia to be more hypnotic and more diuretic than atropia.

Very rarely there occurred a rash on the face and forearms not unlike measles.

Small doses of hyoscyamia at first reduced the pulse-rate, increasing the arterial tension, then excited the circulation. The axillary temperature fell 0.7° F.

Large doses caused immediate rise of pulse; delirium, motor paralysis, sleep, and produced diuresis.

Dr. Lawson's conclusions in his second or clinical study are :

H. is useful in cases of insanity with delusions of suspicion and in mischievous cases.

Remarkably good results are obtained in the status epilepticus.

In the excitement of general paralysis large doses, $\frac{3}{16}$ and $\frac{1}{2}$ grain, are required and do good.

Small doses are efficacious in chroea and in locomotor ataxia.

H. is useless in acute melancholia, in acute mania with delusions of suspicion, and should be avoided in furious mania. As results of the long-continued use of small doses of H. he observed impairment of appetite, but not much dryness of the throat. He employed Merck's amorphous alkaloid (extractive) given in the following formula :

℞ Hyoscyamiæ, gr. $\frac{3}{4}$.
 Spt. etheris, ℥vi.
 Alcoholis, ℥xviii.
 Aquæ font., ad $\frac{2}{3}$ ii.—*ft. haustus*.

I might also mention that Dr. W. Bevan Lewis, Assistant Medical Officer to the West Riding Asylum, in a paper upon "Calorimetric observations upon the influence of various alkaloids on the generation of animal heat," published in W. R. Asylum Medical Reports, vol. vi., for 1876, makes the following statement respecting hyoscyamia (page 51):

Hyoscyamia in small doses diminishes heat-formation, while in large doses it greatly increases it.

Gubler, "Leçons de Thérapeutique," Paris, 1877, pp. 138-9, refers very briefly to hyoscyamia. It causes less delirium and more often a natural sleep than do atropia and daturia.

Refers to Oulmont's use of H. for various tremors.

Denies that it is to be had in a pure state.

Nothnagel und Rossbach, "Handbuch der Arzneimittellehre," 1878, p. 666, state that its physiological action is similar to that of atropia, and refer to its use in tremors. They say nothing of its use as a narcotic. The dose is the same as that of atropia.

Wood, "A Treatise on Therapeutics," 1880, does not state doses for internal administration. He does not believe that any difference between the action of belladonna and hyoscyamia has been proven.

Bartholow, "Materia Medica and Therapeutics, 1880, gives a fair account of physiological actions of H., but is not sufficiently explicit in giving doses of the two kinds. Strangely enough he classes H. among excitomotors."

Stille and Maisch, "National Dispensatory," 1879, pp. 747-9, give a good account of hyoscyamia, clinical and physiological. Its action on the spinal cord is paralyzing, like that of conium. They refer to Lawson's use of it in mania. Dose .001 repeated. In violent excitement up to .06.

A more recent article is one by Dr. Geo. H. Savage, of Bethlehem Hospital, in the *Journal of Mental Science* for July, 1879. Dr. Savage does not like hyoscyamia in the treatment of psychoses, but apparently largely because of certain preconceived general views against neurotics. He admits the quieting and hypnotic influence of the crystallized and the amorphous hyoscyamia, and he gives preference to the amorphous. The former he gave in doses of $\frac{1}{25}$ or $\frac{1}{16}$ grain, and the latter in doses of $\frac{1}{8}$ to 1 grain. In many of his cases anorexia was produced; in a few, collapse. In melancholia harm is done by hyoscyamia. This alkaloid is only good to secure temporary quiet and sleep.

Lastly, Dr. Prideaux, Assistant Physician to the Friends' Retreat, York, in several numbers of the *London Lancet* for

September and October, 1879, has summed up his experience with this drug. He noted the usual differences between small and large doses, viz.: small doses reducing the pulse-rate and cerebral excitement, followed by motor paralysis and sleep; large doses cause an immediate acceleration of the pulse, reduce the temperature, and produce profound sleep. Death may be produced by paralysis of the respiratory centre. Dr. Prideaux used hyoscyamia in several forms of mental disorder, and in the status epilepticus, and reached the following conclusions:

1. In most cases of mania, or where there exists great excitement of any aggressive or destructive character, or rapidity of movement and speech, the use of the drug is the most effectual and rapid means of exercising that form of restraint which has been known as "chemical restraint."

2. That in cases of acute mania it will produce sleep and quietude when all other drugs have failed, and is one of the most rapid and reliable narcotics which we possess.

3. That in the treatment of the epileptic status in epileptic mania, it diminishes the number, frequency and severity of the attacks, especially if its administration be extended over some time.

4. That in delusional insanity, especially in the mania of suspicion, and in other forms of mania where the delusions are varying and changeable, it has a decided action in producing such an altered condition of the cerebral status that a condition which has been called "physiological mania" results, and this so eclipses the former delusions and hallucinations that they are forgotten, and the mind becomes clear; while if the subjection to the influence of the drug be continued, it ultimately leads, under favorable circumstances, to a permanent condition of quiescence and restoration to a healthy frame of mind.

5. That in chronic dementia, associated with destructive

tendencies, bad habits and sleeplessness, the condition of the patient much improves after a continued course of small doses of the drug.

Dr. Prideaux also recommends that hyoscyamia be used in senile mania, delirium and meningitis.

He used Merck's extractive hyoscyamia, and gave from $\frac{1}{16}$ to $\frac{1}{2}$ and even 1 grain in solutions by the stomach.

II. CLINICAL EXPERIENCE WITH HYOSCYAMIA AS AN HYPNOTIC.

Dr. J. C. Shaw, Medical Director of Kings County Lunatic Asylum at Flatbush, has made a somewhat extensive trial of hyoscyamia in cases of insanity. The following is his summarized report to your committee :

I have always used Merck's amorphous alkaloid.

So far I have used the medicine in about fifteen cases.

Acute and subacute mania is the condition in which I expected most benefit from its use. I have also used it in chronic mania with excitement and destructive habits, in the excitement of general paretics, in the maniacal excitement following epileptic seizures, etc. This drug is of great service in acute mania where the patients, from loss of sleep and continued motion, soon become exhausted, and then follows a disagreeable train of symptoms, such as dry tongue, sordes on teeth, and perhaps diarrhœa. If the patient refuses food and has to be fed with the tube, this makes the state of matters still worse. Hyoscyamia is a remedy which obviates this very largely ; it is very certain in its action ; I usually begin by giving $\frac{1}{4}$ to $\frac{1}{2}$ grain by the mouth.

The following cases are given in a condensed form as illustrations :

CASE 1.—Female, aged 34. Admitted Dec. 4, 1879, to the Kings County Insane Asylum. Married ; had one child 9 years ago. The attack has lasted two months. She has delusions of persecution ; attempts to injure herself and others ; talks inces-

santly in a loud voice ; is incoherent ; will not converse or answer questions ; her general health is poor ; she looks thin and pale ; pupils and articulation normal ; tongue clean ; does not sleep ; talks and screams all night ; ceased to menstruate two months ago, just as attack began. Dec. 6th, she begins to menstruate ; Dec. 7th, she has become still more noisy and restless ; breaks windows and furniture, and strikes herself up against other patients ; throws herself on the floor or up against the wall, evidently with suicidal intent. Two days after, the 9th, she has to be fed ; tongue and lips have become dry ; she talks and moves about continually. To-day, for the first time, we give her $\frac{1}{2}$ grain hyoscyamia at 12 M. In one-half hour she was quiet. At 3 P.M. she was sitting upon a bench apparently fast asleep ; eyes closed and head resting on chest ; the cheeks puffed out at each expiration ; pupils dilated ; she was profoundly under the influence of the drug. She slept until 2 P.M. of that night ; at 8 P.M. of the same night she was fed with the tube, but went to sleep again immediately after it. When she awoke in the morning she again became noisy ; passes her urine in her clothing. Dec. 10th, was fed with tube and $\frac{1}{2}$ grain hyoscyamia given ; at 12 she was quite quiet, and ate a hearty supper that evening.

Dec. 11th. $\frac{1}{2}$ grain given this forenoon, and she slept the greater portion of the day. She ate her dinner, but would not eat supper. As she is feeble, tongue and lips dry, she is fed. At 10 P.M. she is quite noisy ; she is given 20 grains chloral and extra night-watch placed over her. She throws herself out of bed, and tries to strike her head against the floor and wall.

Dec. 12th. She slept last night ; to-day $\frac{1}{2}$ grain hyoscyamia is given her ; she slept nearly all day ; she will eat some of her meals ; tongue is now moist and clean ; she spits at every one who goes near her, and will not answer any questions.

Dec. 13th. The $\frac{1}{2}$ grain dose appears to have such a profound effect upon her that we give her $\frac{1}{4}$ grain, which appears to have almost as much effect as the $\frac{1}{2}$ grain did. She slept almost all day ; at night, gave her 20 grains chloral and a bottle of ale. She did not sleep ; restless, and talked all night.

Dec. 14th. Is noisy and running about. I feed her with the tube myself, and give her $\frac{1}{4}$ grain hyoscyamia ; tongue is moist and clean ; she was quiet after the medicine, but did not sleep.

Dec. 16th. She did not sleep last night and was restless ; so this morning she is given $\frac{1}{4}$ grain hyoscyamia, after which she slept.

Dec. 17th. Noisy and excited ; gave $\frac{1}{4}$ grain hyoscyamia.

Dec. 18th. Removes her clothing and is noisy ; $\frac{1}{4}$ grain given.

Dec. 19th. Eats much better ; has not to be fed ; talks incoherently ; $\frac{1}{2}$ grain hyoscyamia given at bedtime in bottle of ale. Slept all night.

$\frac{1}{4}$ grain hyoscyamia is given every day until Dec. 27th, producing its physiological effect each time.

On Dec. 27th none is given.

Dec. 28th. $\frac{1}{4}$ grain given, and every day after until Dec. 31st.

She is much improved physically ; eats her meals ; is quiet.

The hypnotic effect of the drug is here well shown by this case, and its great superiority to chloral in that respect.

CASE 2.—Male, aged 36. Admitted Nov. 8th. Certificates state that he declares that he is the Supreme Being ; the Virgin Mary is his wife and mother. On admission is very noisy, shouting, clapping his hands, walks about incessantly, and behaves in a very demonstrative manner ; is incoherent ; talks about the priests, Virgin Mary, etc., in a rambling manner.

He was given $\frac{1}{2}$ grain of hyoscyamia, and was quiet in 15 minutes, and the full physiological effects of the drug is obtained ; he sleeps profoundly for hours ; as soon as he escapes from under the influence of the drug he is again noisy.

November 12th and 13th he is given $\frac{1}{2}$ grain night and morning ; he is noisy just as soon as the effect passes off.

It will suffice for me to say that the effect of the drug became more and more transient, and the dose was increased until on December 2d, he took, at 8 A.M., 4 grains at one dose, the effect passing off so rapidly that at 3 P.M. he was again as noisy as ever ; at 7 P.M. he is given 4 grains more, making 8 grains of hyoscyamia in one day. He no longer sleeps under the influence of the medicine as he did when it was first given ; the medicine is discontinued as there is no permanent effect. During the time he was taking the large doses he passed large quantities of pale urine, which ceased as soon as the medicine was discontinued.

December 8th, he is given 1 grain hyoscyamia (there has been an interval of 8 days since he took the last dose, and he has been just as noisy and restless as he ever was, night and day) at 3 P.M., which in half an hour produced its marked physiological effects ; but the effect of the subsequent doses becomes less and less marked, until on December 29th he takes 3 grains in the morning and 3 grains in the evening ; 6 grains in the day, and as he has become so tolerant of it, it is again stopped.

CASE 3.—A gentleman who had suffered from epilepsy for years past, has attacks of maniacal excitement after the fits, which will last 3, 4 days or a week ; nothing has ever arrested these maniacal attacks. He has a fit, and a few hours after he begins to shout psalms and praises to God. I gave him $\frac{1}{2}$ grain hyoscyamia at 10.50 A.M. ; at 11.20 he is unable to stand ; at 11.40 can sit up no longer, and has to lie down, and immediately sinks into a deep sleep, occasionally snoring ; pulse 88, increased reflex action (pulse was 112 before he took the medicine), pupils slightly dilated. He sleeps until 6 P.M., when he eats his supper and goes to bed ; he has to be assisted to his room, as he is unsteady on his feet ; he sleeps all night soundly ; the next morning is quiet, and in the afternoon is returned to the quiet hall. The attack would have lasted many days had we not given the hyoscyamia. This is one of the best results I have had with the drug.

I have tried it in some cases of melancholia with excitement, but without special benefit.

CASE 4.—Dementia following mania in a man of 50. He was in the asylum 20 years ago ; was admitted this time about a year ago. He was in the habit of tearing up his clothing, bedding and bed ; in fact, anything he could get hold of. If his hands were restrained he would use his teeth, kick the walls and break the furniture.

I gave him 1 grain hyoscyamia ; in half an hour after he could not stand properly, walked about and tried to pick things off the floor, but which he was often unable to do ; he staggered as if he was drunk. He was evidently conscious that there was something the matter with him, for when one of the attendants told him that he was drunk he laughed. In one hour he was completely unable to stand, but lay down on the floor and went fast asleep.

On the two following days we gave him $\frac{1}{2}$ grain dose each day and then stopped. He never tore up any more things, and became one of the most quiet and orderly men on the hall, and would help to do some of the work.

I have used it in numbers of other cases, and the effects have been alike throughout.

The effects are temporary as a rule ; most patients become quickly tolerant of it, and the dose has to be increased. There does not appear to me to be any deleterious effect produced by its use.

In acute maniacal attacks found inside and outside of asylums, it is undoubtedly a most valuable medicine, and certain in its action.

CASES REPORTED BY DR. A. B. BALL.

The two cases here reported illustrate the marked efficiency of hyoscyamia as an hypnotic in acute and subacute mania, the influence of the drug in controlling delusions, and its occasional tendency to paralyze the bladder.

CASE I.—The first case was one of acute senile mania in a gentleman sixty-five years of age, who had been an epileptic for years, but had had no convulsions for fourteen months. For several months albumen and casts had been occasionally detected in the urine, and granular kidney had been suspected. During the attack of mania, to be described, the urine was normal in quantity, free from albumen and casts, and no uræmic symptoms were present. On Oct. 5, 1879, after having been restless and sleepless at night for three days, he suddenly became very violent, extremely voluble, and imagined he had been attacked and beaten by robbers. This delusion continued through the attack. Pulse and temperature normal for first three days, but both rose on the fourth day, and for the next fortnight the pulse ranged from 115 to 140, and temperature from 101° F. to 103. 4° F. On Oct. 11th he became comatose, and remained so for forty-eight hours, death being almost hourly apprehended. He recovered from this condition, however, and gradually gained strength, but the delusions continued as active as ever. Chloral, bromide of potassium, and morphine were used to procure sleep, but with very unsatisfactory results. On Oct. 25th one-sixth of a grain of Keith's hyoscyamia was given at eight P.M. and repeated at nine. After the second dose, without any noticeable physiological effects, except the production of sleep, patient slept quietly for fourteen hours. Awoke more composed and rational. The same doses were repeated the following night, and on awaking after twelve hours' quiet sleep, the restoration of the mental faculties was complete. When closely questioned in regard to business matters, past events of his life, the multiplication table, etc., his replies were entirely satisfactory. He remembered the events of the first three and last three days of his illness, but the rest was a complete blank.

No relapse occurred, and he has since remained in perfect mental health. During the entire illness the patient was attended by Dr. Alonzo Clark in connection with myself, and was also seen in consultation by Dr. E. C. Seguin.

CASE 2.—The second case was one of melancholia of several months' duration in a gentleman about 70 years of age. Early in the morning of Jan. 5, 1880, after having been unusually restless by day and sleepless at night for several days, he attempted suicide by jumping out the second story window. Falling on the balcony of the story beneath, he escaped injury beyond a few trifling bruises. During the following day he was very restless and had delusions of persecution. At night one-third of a grain of Merck's amorphous hyoscyamia was given, and patient slept quietly for ten hours. On awaking he was much confused, staggered in his gait, and was unable to pass water. Catheter used. The symptoms of muscular paresis passed off in two hours. Patient more composed and rational. One-sixth of a grain given the next night. Slept six hours. Retention of urine again, but has general muscular paresis. Mental condition much improved. Increased appetite. During the next few days he became more tolerant of the hyoscyamia, and half-grain doses were required to produce sleep, but as the medicine always produced retention of urine, it was stopped, and on Jan. 15th 15 grains of chloral with 30 of bromide of potassium were substituted. A more restless night. At eight o'clock A.M. he sat up in bed and ate his breakfast, after which he lay down in bed and fell into what was supposed to be a deep slumber. At nine o'clock he could not be roused, and a few moments later died. The cause of death was uncertain, as no *post-mortem* was allowed. Dr. Abram Du Bois attended the case, throughout the illness, in connection with myself.

A CASE OF DELIRIUM TREMENS, BY DR. F. P. KINNICUTT.

The attack occurred in a gentleman aged . . . The treatment was initiated on November 26, 1879, and consisted of large doses of chloral and bromide of potassium, with unsatisfactory result in producing sleep; not more than two hours' sleep being obtained at any one time, and that of a restless character. During the night, November 1st and 2d, he took 75 grains of chloral and 120 grains of bromide of potassium without producing more than a few moments' restless sleep at any one time.

On November 2d. The patient's condition is wretched. He is extremely restless, depressed, threatens suicide and even made an attempt; begs for stimulants. Hyoscyamia $\frac{1}{60}$ grain (Merck's crystallized preparation) is given by the mouth and ordered repeated *pro re nata*.

November 3d. Passed a very good night; sleeping several hours and very naturally. One-fiftieth grain had been repeated in the afternoon of the 2d, and $\frac{1}{100}$ before 10 P.M. From this date there was continued and rapid improvement; good nights with apparently physiological sleep. One-fiftieth grain administered by the mouth twice a day was found to be sufficient.

Aside from hypnotic action of the drug, the only effects observed were hallucinations of a pleasant kind, and slight dryness of the throat. No alteration of temperature occurred after doses.

CASE CONTRIBUTED BY DR. ANDREW H. SMITH.

I was consulted by letter on January 5, 1880, in regard to the following case:

E. T., aged 65. Three of his brothers and sisters died insane, two of them by suicide. For the past two years he has been gradually developing melancholia, recently becoming rapidly worse. He was very restless, sleepless, unmanageable, wandering about the house, talking wildly and excitedly. Pending his removal to an asylum I advised the administration of $\frac{1}{60}$ grain of crystallized hyoscyamia to be given night and morning. This produced sleep at night and rendered the patient quiet and manageable during the day, to the great relief of the family, to say nothing of himself.

CASE OF MORBID DREAMS, BY DR. E. C. SEGUIN.

A gentleman, 50 years of age, affected with paræsthesiæ in the head and tinnitus aurium, was sent to me for treatment by Dr. J. S. Jewell, early in November last. Mr. X. was unquestionably hypochondriacal to a certain extent. Many years ago he had been a dyspeptic; he had worked unremittingly at his business for many years, and in the last ten years his head had become worse and worse; insomnia had developed and he had led a wretched existence. His insomnia was made the more trying because what little sleep he obtained was troubled by fantastic and fearful vis-

ions of a panoramic character ; scenes of violence, obscenity, comicality, would, as it were, pass before him almost with the vividness of hallucinations. Indeed, he had the same "visions" at any time if he dozed for a minute in his chair. Treatment began November 14, 1879. Injections of 3 grains of camphor failed.

On the 15th, I gave him, chiefly with the idea of procuring sleep, the crystallized hyoscyamia in doses of $\frac{1}{6}$ grain, by hypodermic injections, at bed-time.

19th. Much better ; little pressure in head for four days ; less tinnitus ; some sleep. At night, $\frac{1}{3}$ grain of hyoscyamia hypodermically.

21st. Depressing and fearful dreams are much less marked, though there was not much sleep last night. Given $\frac{1}{5}$ grain hyoscyamia and $\frac{1}{4}$ grain sulphate of morphia under the skin.

22d. Had a good night.

On the 29th he was ordered one tablet of $\frac{1}{60}$ grain hyoscyamia at bedtime with opium. The peculiar dreams have almost ceased. After this date, to the present time, Mr. X. has used one tablet of H. almost every night, and has had ugly dreams only on one or two occasions. He has used small doses of McMunn's elixir of opium (morphia and opium in substance caused great itching) and a varied restorative treatment, with marked influence upon all his symptoms.

A parallelism might, I think, be drawn between this arrest of morbid dreams and the happy effect of hyoscyamia in cases of delirium tremens, delusions of persecution, etc.

EDITORIAL DEPARTMENT.

HIGHER MEDICAL EDUCATION IN NEW YORK.

The last twenty years have witnessed a steady and usually intelligent improvement in the curriculum and requirements for graduation of the various medical schools in New York. A number of devices, short of a radical reform, have been resorted to, in order to insure a better education to the numerous youths who, often after no proper preparation, wish to become physicians. Some of these proposed reforms are more or less illusory in their nature, and read well in prospectuses, but others are of unquestionable sincerity and value, such as the lengthening of the regular term of lectures, the opening of laboratories, and the enforcement of more rigid examinations. The offer of substantial prizes to the members of the graduating class who shall come out best of two competitive examinations, if not theoretically correct, seems a beneficial stimulus. Besides these efforts on the part of the various schools, the enterprise of a number of well-trained physicians offers to students (and to graduates) quite varied opportunities for private practical instruction. Among these we cannot, however, include the so-called "quizz-classes," whose influence must be deemed pernicious.

In the midst of these indications of a somewhat reluctant though encouraging progress in elementary medical instruction, we fail to perceive any attempt at serious post-graduate education, and, practically, many of the great advantages which a metropolis offers for the pursuit of the higher medical studies and for orig-

inal investigation are not utilized in New York. Young American physicians who can afford the expense must go to European cities to find that thorough clinical and experimental instruction which is necessary to the extension of their training.

We certainly have hospitals and dispensaries and colleges enough to give a field for post-graduate study. We have, taking the city as a whole, probably as many cases of the rarer forms of disease, medical, surgical and special, as are necessary to illustrate such study, and we believe that there are enough men well qualified to direct higher medical studies.

With the material and the teachers, why is it that so little is done in this great city for the advancement of scientific medicine? Why must a costly European tour be necessary (apart from its nominal advantage) to the completion of a medical education?

The reasons are numerous, but one fundamental reason stands out so prominently as to deserve elaborate consideration. Were the defects we are about to enumerate to be remedied, New York would soon acquire preëminence as a scientific medical centre, instead of being chiefly known, as it now is, as a place where so many hundred young doctors are manufactured every year.

This chief obstacle to progress is, we firmly believe, the bad organization of the medical staffs of our hospitals.

1. The *personnel* of the hospital staffs is most unequal and heterogeneous. The lower grades of internes are, as a rule, filled by unusually intelligent and well-qualified recent graduates who have passed a competitive examination, which for some hospitals is very severe. The higher grades, those of visiting physicians and surgeons, the places of the men who are to have the responsibility of the treatment of patients, and who are supposed to train the internes, are filled without any examination or other test of capacity. Through political, or social, or collegiate influences the directing authorities of hospitals select these visiting physicians and surgeons. It thus happens that some competent men are appointed, as well as many others who are both incompetent and ignorant.

Thus it happens that in a hospital ward, the well-prepared junior medical men must go around under the nominal direction of a visiting physician or surgeon who is intellectually their inferior, and who, perhaps, knows much less of medicine. This relation, which has been known to exist in our hospitals, is hardly a favorable one for hospital discipline, or for the education of the junior staff, and as visiting physicians and surgeons are on duty from two to four months at a time, it follows that the internes are witness to the most varying exhibitions of knowledge and capacity, and they must employ a portion of their time in criticising or contemptuously discussing the conduct and qualifications of those who should be their teachers.

Besides, the number of visiting physicians and surgeons is too large, and that of internes is too small in proportion to the number of patients.

The following table is constructed from data obtained from the Medical Register for 1880. It embraces only the chief institutions :

Hospitals.	No. of Beds.	Attending Staff.	Proportion.	Resident Staff.
Charity	1,000	27	1:37	8
Bellevue	800	26	1:30	8
Presbyterian . .	100	10	1:10	2
St. Luke	209	8	1:26	2
New York	150	9	1:16	2
St. Vincent . . .	250	6	1:40	1
St. Francis . . .	200	6	1:33	2
German	90	10	1:9	1
Mt. Sinai	160	7	1:23	2
Roosevelt	180	7	1:26	2
Nursery and Child's	32	4	1:8	1
Woman's	127	6	1:21	2

Each resident staff is usually made up of three internes, viz.: a house physician or surgeon, and two assistants; in some hospitals only one assistant. The duties of the house physician or surgeon are very responsible and onerous, and the proper performance of them becomes difficult when he has more than fifty beds under his supervision, and, in my opinion, impossible with more than one hundred beds. He is supposed to know all about each

patient, making the morning visit to all the wards, prescribing for the patients under the supervision of the visiting physician, and directing his assistants in the performance of their duties.

Histories of all the patients are kept, or are supposed to be kept, and this alone is a great work when the number of patients exceeds fifty. The numerous exact medical observations and elaborate surgical dressings which are now necessary to successful practice are nearly all to be done by the resident staff, and consume much of their time.

As things now are I believe that our hospital case-books would hardly stand a critical examination; fragmentary, unscientific notes of cases too often taking the place of what should be an accurate account of a case. The responsibility for this defect, a very serious one, as it interferes with the use of hospital records for study and publication, is to be shared by the visiting and the resident staffs: the former not taking sufficient pains in dictating notes and reading over the case-books, and also because their short terms of service limits their observation of many cases; the latter being overburdened with various duties, and not sufficiently guided by the visiting physicians. Recently an eminent physician told me that he had had quite a number of cases of a certain rare disease in his various terms of service at a certain hospital, and he intended incorporating them into an article. A few weeks later he told me that the histories of these valuable cases were nearly worthless and he would not dare make them public. This I believe to be a frequent experience, and to be one reason why our hospitals produce so little in the way of original research.

The number of visiting medical officers is much too large; on the average, according to the above table, one to twenty-six patients. Partly for the sake of making a larger "service" for visiting physicians and surgeons, the European practice of having certain wards assigned to each physician for years has been abandoned, and the custom of quickly rotating duty substituted. Thus, in Bellevue or Charity Hospital, a visiting physician is on duty for two or three months, and thus has charge of about one hundred patients.

The Woman's Hospital and St. Francis Hospital are the only ones, to my knowledge, where the physicians have their respective divisions, and serve the year around.

Another evil way of seeking to correct the drawbacks of short services is by having a service in more than one hospital. Several physicians in New York rejoice in the privileges of three hospital appointments.

It is this plan of short services which has, we believe, done the most to lower the utility of our hospitals, both from a scientific and a humanitarian standpoint, and it is a prominent obstacle to higher medical education.

Let us consider some of the ways in which this plan operates injuriously.

It is unsatisfactory to the physician himself. He goes on duty and must become acquainted with the resident staff attached to his division or wards ; that is, to know them in a personal and a scientific manner. It may take him a week or two to find out whether the young gentlemen of the staff are reliable and willing to be guided by him. He may find that, however well-intentioned at the beginning of their connection with the hospital, they have been demoralized, rendered careless, skeptical, or even rude, by the incompetency or carelessness of the preceding visiting physician. It is then necessary for him to learn more or less about all the patients in the wards assigned him, numbering from sixty to one hundred, and this is by no means an easy task. When the time has come for him to abandon his service he begins to understand his cases. It is not merely his advice which is sought, but he is supposed to actually direct and be responsible for the treatment of each case. Some of the cases in the wards are acute and may run their course during his term of service, two or three months ; but many others are chronic, and some of these are of the highest interest. In my experience chronic cases are often neglected and allowed to become incurable in wards which are subject to this rule of short services, partly because the visiting physician naturally prefers to devote his attention and give time to cases which he can watch through

the whole of their evolution. There are many cases of disease which are curable only by six, or eight, or twelve months of systematic treatment persistently directed by one mind; but with a change of physicians every two months what can one hope to accomplish in such cases?

How can a physician who enjoys the privilege of a hospital service for two months once or twice a year, ever hope to utilize his hospital experience? How can he accumulate numbers of well-made observations upon certain diseases, upon certain therapeutic procedures, or how can he ever hope to demonstrate his interest and success in special lines of practice? Can our surgeons ever hope to rival their European *confrères* in the matter of numbers of operations? A discouraging no is the only possible answer to these queries, and perhaps it is the appreciation of the really small value of their services which renders many visiting physicians and surgeons so careless and superficial in their hospital work.

Monumental works, like Trousseau's Clinical Medicine, Murchison on Fevers, Andral's Clinical Medicine, Charcot on Diseases of the Nervous System, would have never been possible with other than continuous hospital attendance and study.

The influence of the visiting staff upon the internes is weakened or perverted by our system. Even in our deficient organization the house physician or surgeon and his assistants are supposed to be men who have entered the hospital for the purpose of further study, to obtain the practical education which colleges do not give; they are pupils. Yet, not only are these young men left, as a rule, to pick up knowledge for themselves, are not trained by any one mind, but they are often led into the delusion that they are not pupils. This demoralization is brought about by the presence of incompetent or careless men on the visiting staff. The internes quickly perceive the ignorance or lack of skill and success of their visiting physician, and after a few visits entertain, or even openly show contempt for him; the house physician is obliged, or thinks he is obliged, to assume a sort of charge of cases, and not very rarely the visit degenerates into a

formality or a farce. Other visiting physicians and surgeons, perfectly competent, come to the hospital, go to the house physician's room, engage in pleasant chat, and ask : " Anything special to-day, doctor ? " or, " Any new case to-day, doctor ? " Then follows a short visit to the ward, a walk around, a word said to one or two patients, and the farce is done. If there is nothing " special," or nothing " new," the visit is occasionally wholly omitted. In consequence, some resident staff vote the physician or surgeon a mighty nice fellow who does not interfere, and go on untaught, seeking experience for themselves.

It may be said that such neglect of duty is not the rule ; perhaps it is not ; but admitting that each hospital has one such visiting physician or surgeon, the demoralizing effect is produced upon the resident staff : the notions of contempt for their professional superiors and of their own relative capacity to " run the wards " enter their minds, and are difficult to eradicate. From conversations with conscientious and able visiting physicians, I know that to take a service after the young men attached to it have got into this morbid mental state is a discouraging experience. The thorough visits, the complete examinations of patients, the requests that minute observations be made, etc., seem to the demoralized resident staff like tyranny and fussiness ; they have forgotten that the patients professionally belong to the visiting physician, and that they themselves are in the hospital to be taught and trained.

Under the European plan of continuous attendance, a young man who enters a hospital has the prospect of serving through various grades under one physician, of being constantly watched and advised by him, of becoming moulded by his methods, of being afterward known as his pupil, and, if worthy, of being helped by him in scientific and practical matters after leaving the hospital. Can any of the many able young men who leave our hospitals say in after life : " I was a pupil of so-and-so ; he trained me, and I have assisted him in his researches ! " They can only claim that they witnessed the various ways of a number of visiting physicians, some able and careful, others able and

careless, others still incompetent or even ignorant ; that they "picked up " many valuable notions, and neglected many fine opportunities for clinical and therapeutical study. They look back with pleasure to their relations with some of their visiting staff, and with mingled contempt and resentment at their subjection to others.

With our system of short services how can a higher medical education be attempted? A physician from a distant place, or a recent graduate of one of our schools, would like to become the pupil of Dr. A——, who is so well known in connection with, we will say, the study of fevers, or for his skill in auscultation and percussion. He would like to watch his cases and methods, and really learn something of the subject. He finds that Dr. A—— is attached to a certain hospital, and further, that he has charge of two or more wards of that institution for two months only—a period allowing of the complete observation of only a few cases ; he finds that this gentleman's wards contain numerous cases of diseases in which neither Dr. A—— himself nor the visitor is interested, and that the class of diseases he came to observe is represented by a few cases. Can he carry out his plan? Is he not, if he can afford it, driven to go to Vienna, or Berlin, or Paris, even though he may feel convinced that Dr. A—— is an able man and fully as good a teacher as the German or French professor he will have to find? But this foreign physician controls a service year after year, and, by a sort of natural selection, cases which he understands best have accumulated in his service, and he can at almost any time offer a rich store of instruction to pupils.

This criticism is true, we believe, of the present state of all our hospitals, except perhaps those for diseases of women and for diseases of the eye and ear. We might amplify the criticism, but perhaps, put in the above brief form, it is fully comprehensible, and we believe its validity will be admitted.

Lastly, by no means least, the system of short medical services, two months or even four, is very unjust and unfavorable to many patients in our hospitals. As stated before, many chronic or semi-

chronic cases cannot receive that continuous and persevering treatment which alone can save them from incurability. Each new visiting physician or surgeon must go over such cases, take time to understand them, and then, probably, he will try another treatment for a few weeks. Or else, for the length of one physician's term of service, the care of these cases will be left wholly in the hands of the resident staff, because the visiting physician or surgeon is interested only in acute cases, or cases requiring operation. I believe that the controlling authorities of our hospitals would be shocked if they knew the number of cases which undergo this "treatment" in their institutions, and also if they realized how many curable or ameliorable cases are refused admittance because they come under the somewhat arbitrary classification of chronic or incurable affections—terms which are wrongly held to be nearly synonymous, especially by hospital physicians.

Another way in which patients in hospitals are aggrieved is this. They have a certain moral right to ask to be under a given physician's care, provided there be room for them in his ward, and the discipline of the hospital be not injured by complying with their request. This right is more evidently possessed by paying patients. If Dr. B, for example, has earned a reputation for skill in the treatment of diseases of the digestive apparatus, a dyspeptic wretch who applies at Dr. B's. hospital should, if he desire it, be given the benefit of that supposed skill. With our present system this dyspeptic, if he wishes to be under Dr. B's. care, must wait until Dr. B "goes on duty," and then may be treated by him for two or three months, when another physician, who, perhaps, is far from skilled in the treatment of dyspepsia, and takes no interest in such cases, assumes charge of the ward. What can be the result of such an experience, but relapse of the disease or failure to completely cure it? Some years ago it was within my personal knowledge that uterine cases were neglected and left wholly in the hands of the house physician by some of the visiting physicians of a hospital in this city. The rules of the hospital allowed of the reception of these cases and their location in the wards of a physician who knew little about them, and who looked

upon the manipulations necessary to their diagnosis and cure with feelings of mingled dread and repugnance.

As I have pointed out in another place,* there is an inevitable, semi-conscious tendency to the cultivation of specialties in medicine, and nearly all of the men from among whom hospital physicians are or should be chosen, are quasi-specialists, *i. e.*, they have studied with care and feel an interest—a real scientific interest—in a limited number of diseases, and in proportion as they increase their knowledge and skill in this special direction, they become less competent to deal successfully with other diseases. The system of short changing services forces all patients to submit to a succession of variously qualified physicians, and obliges a physician to try to treat all sorts of diseases, or tempts him into neglecting many of the patients placed under his care. The system does not recognize the fact, the hard and undeniable fact, that all intelligent and scientific physicians are quasi-specialists, and must be. In the present development of medical science there is no alternative ; a physician must be a quasi-specialist, or possess a universal knowledge of a superficial, mostly booky kind, a knowledge wholly insufficient to ensure intelligent or successful practice.

In this city the height of absurdity in the way of appointing physicians to services for which they have no liking, was reached when a well-known specialist was placed in charge of a service in a hospital devoted to a specialty altogether different from the one in which he excelled ; but every hospital at the present time is witness to a minor degree of the same injustice to patients.

In a subsequent article means of correcting these evils will be discussed.

E. C. SEGUIN.

* The cultivation of specialties in medicine. ARCHIVES OF MEDICINE, vol. iv, p. 280.

NEW BOOKS AND INSTRUMENTS.

Diagnosis and Treatment of Ear Diseases. By ALBERT H. BUCK, M.D. pp. vii, 411. New York: Wm. Wood & Co., 1880.

Judged by its literature, otology is more assiduously cultivated in America than in any other country. We have two quarterly journals exclusively devoted to its interests, and since 1872 have furnished five original text-books on the subject, the last of which is the one whose title heads this notice.

Dr. Buck's book differs in one important particular from its predecessors; it is in no sense a compilation. Authorities are cited, it is true, but the book, in the main, is simply a statement of the author's experience on all the points touched upon. This, it seems to us, is a most commendable new departure in book-making. What we most desire to know when we read a book written by a man of observation and experience, is what that observation and experience have taught him. In most books we have great difficulty in sifting original observation from the observations of others, and from the traditions and opinions that have come down by unproven authority, from text-book to text-book, and are accepted with an undoubting faith as to their correctness. Dr. Buck seems to have taken nothing for granted, and has given his experience with a straightforwardness that is truly gratifying. This is particularly so as regards treatment. He lays down that plan which has yielded him the best results, and if he has not tried thoroughly other plans he says so, leaving the reader to employ them if he should not be satisfied with that which has been most satisfactory to the author. Illustrative cases are supplied in abundance throughout the book.

The first chapter in the treatise is devoted, very properly, to a

short consideration of the physiology of hearing, which, though concise, is yet full enough for the practitioner, be he special or general. The next chapter—on the examination of the patient—contains nothing essentially new. For inspection of the membrana tympani, he uses by preference a short conical Wilde's speculum and a forehead mirror. In the consideration of the diseases of the external auditory canal, we meet with the first departure from the traditions of the text-books. In all former treatises it was laid down as a fundamental principle that in removing impacted cerumen or foreign bodies from the external meatus, no other instrument should be used but the syringe. Dr. Buck says: "Little by little I became satisfied that in the great majority of cases the removal of the mass [of cerumen] could be effected much more quickly and pleasantly by the use of the curette, the cotton holder and angular forceps, than by any other plan. * * The objections raised by some authors against the instrumental plan of removing impacted cerumen, viz. : that it requires great skill and may prove dangerous in unskilful hands, does not seem to me to be a valid one. Therapeutic methods should be recommended in accordance with their absolute merits, and not because they will be likely to prove least harmful in unskilled or clumsy hands."

En revanche, we can with great propriety say that Dr. Buck's objections against the syringe are not valid. There are few cases of impacted cerumen which cannot be removed by the syringe alone if sufficient time and patience are given to the work. There can be no question that the use of instruments in removing foreign bodies from the external meatus *is* dangerous, unless the instruments are in very skilful hands. Our literature is only too full of cases where serious injury and even death have resulted from the attempt to extract, by instruments, foreign bodies from the meatus, which, in some instances, were not there! We have ourselves seen cases where severe inflammation of the external meatus, resulting in caries of the bony walls, has followed attempts to remove more than ordinarily impacted cerumen. When all other means have failed, then we would say instruments should be used, but with the greatest caution, and in this sentiment we are sure we express the feelings of a majority of otologists. Dr. Buck, we doubt not, can use instruments with as little liability to damage to the soft parts as the syringe would incur, yet we cannot but look upon it as pernicious teaching to lay it down as a principle, in a book which, from the circumstances

of its publication, is intended as a guide to the general practitioner rather than the working specialist, that the syringe, in removal of foreign bodies from the meatus, is to be the exception rather than the rule.

In the treatment of inflammation of the external meatus (circumscribed or diffuse) the author has to some extent abandoned the traditions. He is doubtful of the efficacy of incisions in cutting short or essentially modifying the course of these inflammations. They may, however, afford temporary relief. In the main, he relies upon heat—dry and moist,—and he finds the poultice, so long tabooed in such cases, to be a very valuable means for conveying the moist form of heat. In speaking of remedies addressed to the general system, we see no mention made of sulphide of calcium which, a short time ago, was so eulogistically spoken of. Tonics are often indicated in such cases, the patient being most generally “run down.” He speaks of a distinct form of inflammation of the outer meatus under the head of “desquamative inflammation,” which consists in the production of almost continuous sheets of epithelium.

As it is the inflammations of the middle ear which form the bulk of cases coming for treatment, we will pass over the consideration of other subjects to note whatever that is new or important in Dr. Buck's treatise which pertains to these affections. His chapter on acute inflammation of the middle ear (which he divides into catarrhal and purulent) is most excellent. As the majority of cases of acute and subacute catarrh of the drum-cavity are associated with inflammation of the naso-pharyngeal space, he, in the first place, directs his treatment to those parts. As a local application, he has come, from his experience, to use almost exclusively solutions of nitrate of silver, and these he applies by means of a cotton-holder bent at the end almost at a right angle. He is thus enabled to bring the solution with which the cotton is impregnated up behind the soft palate into immediate contact with the parts about the mouth of the Eustachian tube. The strength of the solution he uses varies from 15 grs. to the ounce of water to a saturated solution—seldom, however, so strong as the last. He but seldom uses any of the powders or other mixtures which are recommended. Iodine he thinks next best to silver. Where there are adenoid vegetations in the pharynx they must of course be removed, and where the tonsils are very much hypertrophied he advises their excision. Meddling with the middle ear in any manner, by way of the tube, except, of course, the air-

bath, he advises against. Inflation by means of the catheter or Politzer's instrument is most efficacious. It seems to have a decidedly therapeutic effect on the inflamed tissues, aside from opening the tube and dislodging any mucus that may be accumulated there. Of the two plans of inflation he gives preference to Politzer, and only employs the catheter when Politzer fails. As to the frequency with which inflations should be employed he is in doubt, but seems to have had more satisfaction from four or five successful ones at one sitting every other day. He rarely continues systematic treatment for longer than four or five weeks. After a rest of two, three or four weeks, he again resumes the inflations and applications to the naso-pharynx. This plan of intermittence in treatment we think most admirable, and we are glad to find Dr. Buck laying stress upon it. We are satisfied, from our own experience in such cases, that a too continuous treatment is likely to do harm. The temptation is great to do too much and to do it in too short a time. In the management of such cases the element of time enters most largely. As regards general measures there are two points which are deserving of attention, viz.: change of air and scene, and attention to the digestive apparatus. In the consideration of special stages or conditions, he speaks of paracentesis of the membrana tympani. Formerly he incised the membrane in almost all cases, but he has found that in the majority of instances his cases ran about the same course whether the membrane was punctured or not, and he now limits paracentesis to those cases where, after three or four weeks, there is no diminution of fluid in the drum-cavity. We are ourselves of the same opinion as Dr. Buck, and think that the systematic puncturing of the drum, which just now seems to be the fashion, is, to say the least of it, often a needless procedure. Minute directions are given for the performance of the operation.

Barely three pages are given to the consideration of what the author calls "chronic subacute catarrh." There may be a subtlety of meaning in this apparently paradoxical term, but we are of the opinion that some of the other terms applied to the condition he describes would have conveyed a more definite idea of what we know constitutes about thirty per cent. of the cases applying to the aurist for relief. If we are to have a contradiction in terms let us call it "dry catarrh," because if it is a catarrh (which is doubtful), it certainly is "dry" as compared with the mucous and purulent forms. It is true the author makes three divisions, in which the condition of the mucous membrane

as regards congestion is taken as a basis, but in none is there any secretion. They all have this in common, that there is progressive deafness, noises in the ears, and an intact drum-membrane, and there is but little, if any, improvement from treatment.

We are glad to see a writer have the courage to declare his inability to give relief except to a small majority of such cases. He says: "We may then proceed to carry out the plan of treatment recommended on page 165 and following pages, and if, by the end of say the third week, we shall not have succeeded in accomplishing at least the beginnings of a favorable result, we may without hesitation abandon our efforts" (p. 195).

Chapter vii, which is devoted to the purulent forms of inflammation of the middle ear, is a long and very valuable one. It deals with the acute and chronic varieties, under which latter head falls, of course, otorrhœa. The gravity of the acute form of purulent inflammation of the middle ear will be realized, when we remember that "a localized meningitis may be assumed to exist in every severe case of acute purulent inflammation of the middle ear."

The propriety of early and prompt treatment and the prognosis are summed up in the following sentence: "If begun soon, *i. e.*, within a few days after the onset of the attack, an acute inflammation of the middle ear rarely results in the patient's death, or seriously impairs the hearing of the affected ear, or passes into a really chronic purulent inflammation."

In the treatment of the acute form of purulent inflammation, a somewhat different plan is called for from that used in the acute catarrhal form, particularly as regards paracentesis of the drum-membrane. Here, as soon as the m. t. becomes bulging, it should be incised, but not before. In cases where from swelling of the adjacent parts we cannot decide whether the membrane is pushed outward, we should give the patient the benefit of the doubt, and puncture. The other means of affording relief and shortening the course of inflammation are dry and moist heat. A constant stream of hot water, kept in contact with the inflamed drum-membrane by means of Clarke's douche, is probably the most efficacious. Other plans are bags of hot bran, salt, or simply hot water poured into the ear and renewed as often as it becomes cool. Buck places local bloodletting next in importance and value to the preceding. The leeches (never less than four for an adult) should be applied to the external meatus or just behind the auricle.

The subject of otorrhœa is one which is of special importance to the practitioner. "Discharge from the ears" was, until within quite modern times, the opprobrium of the profession. In fact, it is only since the improved methods in diagnosis have led to thorough examination of the middle ear and the discovery of the causes lying back of the discharge, that its rational treatment has become possible. To effect this complete examination, the first and most important step is a thorough cleansing. When one has mastered the art of properly cleaning the ear, the author is "almost tempted to say, he will find himself a master both in the diagnosis and treatment of diseases of the ear." In this cleansing, the syringe is not to be used, as a rule. In this limitation of the use of the syringe we are in hearty accord with the author. One is little likely to do damage with the end of a cotton-holder well covered with cotton; and besides, no amount of syringing will remove tenacious mucus which is hid away in the "cracks and crannies" of the middle ear and among polypi and granulations. After the ear is well cleansed, the next step is to make oneself thoroughly acquainted with the topography of the bottom of the meatus. This can be done only by means of the probe.

A simple inspection can give us no definite idea of the relation of things. By the gentle use of the probe we find the position and attachment of polypi, the consistency of granulations, the presence of sinuses, etc. The author is no doubt correct in saying that the spores of the various forms of *aspergillus* are often conveyed into the ear by syringing, and there find a prepared nidus in the pus which is ever present.

In every case of long-standing otorrhœa, the author thinks it is a good plan to assume "that the discharge is kept up either by a carious process in some portion of the temporal bone, or by a mass of granulation tissue situated in one of the following three places: the external auditory canal, the middle ear proper, or the mastoid antrum." He does not, therefore, believe that chronic otorrhœa frequently comes from a simple inflammation of the mucous membrane—in which he is probably correct,—and, as a consequence, in his treatment he directs attention almost entirely to a removal of these causes. We do not notice any important consideration of the dry treatment of otorrhœa, which comes to us so highly recommended quite recently, and evidently for the reason that the author is content with his success in his own plan. This plan consists, for the most part, in a thorough removal of all granulation tissue by the snare (Blake's) and touching the base of the pedicle with nitric acid.

His plan of treatment of purulent inflammation of the drum-cavity, where there is not a large perforation of the m. t., is most excellent ; but we have no space left in which to justly consider it. This and the chapter on mastoid disease should be read by all specialists. They are exhaustive, and on the latter subject Dr. Buck can speak *ex cathedra*.

On the whole, we would say that Dr. Buck's treatise is the most important one, for the specialist, that has been issued in this country ; but the plan of its publication in a series will, we fear, keep it out of the hands of those whom it would most benefit.

As is the case with all of "Wood's Library of Standard Medical Authors," its "get up" is good ; but we must enter our protest against the absence of running titles to the chapters, at the top of the pages, in a book which will so frequently be used as one for reference.

[S. M. B.]

A Treatise on Diphtheria. By A. JACOBI, M.D., Clinical Professor of Diseases of Children in the College of Physicians and Surgeons. New York : William Wood & Co., pp. 252.

The literature of diphtheria has been enriched during the past few years by many valuable contributions. Not to mention innumerable articles, pathological and clinical, in the medical journals, the chief of these have been Oertel's work in Ziemssen's Cyclopædia, the report of the Committee of the Royal Medico-Chirurgical Society and the discussion thereon, and the treatise of Morell Mackenzie.

The cause of activity in this special branch has been owing, in part, to the prevalence of diphtheria during the past decade, and, in part, to the increased zeal with which the investigation of epidemic diseases has been carried out.

The profession will heartily appreciate this work of Dr. Jacobi's, as it embodies the experience of a man who has made a special study of many thousand cases of diphtheria, and has already published important memoirs on the subject. It is stated by the author, in the preface, that this book may be considered as an augmented edition of his article in Gerhardt's "Handbuch der Kinderkrankheiten," with this exception, that it contains less literature and more therapeutics. The fact that 80 of the 252 pages are devoted to treatment shows the truth of this statement, and illustrates also the extremely practical character of the work.

The first chapter, on the history of the disease, contains a most

interesting sketch of the early epidemics and the gradual advance of our knowledge up to the present time. The disease appears to have troubled the colonists very early in the settlement of New England. In 1659 three of the children of Samuel Danforth were attacked by the "malady of bladders in the windpipe," and "it pleased God to take them all away at once, even in one fortnight's time." Several extracts are given showing the prevalence of the disease in America during the seventeenth and eighteenth centuries. It is stated that Samuel Bard, an American physician (1771), was the first to prove the identity of all forms of diphtheria, 50 years before Bretonneau's observations, and his account of the disease appears to merit the encomium passed on it by Dr. John C. Peters, who says that his article "is among the calmest, wisest, and most accurate that has ever been written on diphtheria both before and since his time." Some of the paragraphs on treatment, extracted from the writings of these old physicians, might have been transcribed from any modern work. Thus, Dr. Jacob Ogden recommends "detergent gargarisms or injections," the impregnation of the air of room with the steam of vinegar, and disinfection by means of burning sulphur.

The points in connection with this disease, which are of special interest to the profession, are the parasitic theory of its origin, its relations to croup, and the methods of treatment, and to these, as dealt with by the author, we shall mainly refer.

The parasitic theory has been much insisted upon of late, and the emphatic statements of Oertel and other German writers have caused many to regard it as firmly established. The simplicity of this view has also been in its favor. Local infection and growth of the membrane under the irritation of the bacteria, penetration of the parasites to the deeper structures, with general infection of the body; what easier explanation could there be? The number and extent of the observations on this point are extraordinary, especially in Germany, where the medical journals for the past ten years have teemed with reports of cases and experiments illustrating the bacteric origin of the disease. The ubiquitous micrococcus spared no organ, and wherever found the process was necessarily diphtheritic. If in the kidneys, even though there was no evidence of infection elsewhere, still it was a diphtheritic nephritis. Letzerich describes (in his own child) a diphtheritic encephalitis! The position taken by Dr. Jacobi on the question is very satisfactory, and may be gathered from the following quotations: "This much is positive at any rate, that no one has yet

proven that the vegetable organisms alone, and not other, free or fixed, parts of the diphtheritic membrane are vehicles of the infecting elements; and even now the question has not been decided whether the bacteria met with in diphtheria constitute the cause of the disease, or are a part of the process, or co-effects of the poisonous action; whether they are the carriers of the poison, or entirely indifferent entities" (p. 38). "There are schizomycetæ, more or less, in most cases of diphtheria and other infectious diseases. But it is not proven, but only claimed that they are essential. Even when micrococci, by immense accumulation in certain localities, result in a local necrosis of the tissue, it may be claimed, but it is not proven at all, that this effect of the parasites is the essence of diphtheria" (p. 49). "At present it seems altogether improbable that bacteria have any direct function in diphtheria, *i. e.*, that they enter the system as bacteria and develop as such in the system, and cause the symptoms" (p. ix, preface). The truth appears to be that a number of pathologists have been so enthusiastic over the idea of at last getting the actual germs of disease and being able to cultivate and label them, that their zeal has got the better of their judgment, and "many promises which were made could not be fulfilled, and many proofs which were offered could not be presented." An amusing victory for the parasitists is recorded, which shows that, in Germany at least, this theory has extended to the laity. In a forensic case lately tried, an unlicensed practitioner was found guilty of "having permitted, by his therapeutical procedures, the bacteria to enter the body of a patient and caused her death thereby."

The able investigations of Curtis and Satterthwaite, of New York, and Wood and Formad, of Philadelphia, are largely quoted, and with reference to the latter, the author states, that "the mooted question of the essentiality of bacteria in regard to the nature and definition of diphtheria . . . is finally settled by them."

Attention is called to the possibility of contracting the disease from the lower animals, and some interesting facts are given bearing upon this point. Damman's cases of infection from calves are referred to, and the disease is stated to prevail among domestic fowl. An instance is given, at Ogdensburg, of three kittens taking the disease from children ill of diphtheria. We have been told by a medical man of an outbreak of diphtheria in a family, which followed closely upon the death of a pet cat from a throat affection which he believed to be diphtheria. We have met with a case of rapidly fatal diphtheritic laryngitis and enteritis in a cat.

As regards the anatomical characters, the author states that we have to deal with three different manifestations of the process : first, with a membrane lying on the mucous membrane, and removable without causing much injury to the epithelium or to any of the basement membrane ; such membranes were given by some the name of croupous deposits ; secondly, with a membrane implicating the epithelium and upper layers of the mucous membrane ; to this, the title of diphtheritic membrane has been given, by preference ; thirdly, with a whitish or grayish infiltration of the surface and deeper tissue, which, if abundant, may give rise to a necrotic destruction of the tissue. The character of the membrane will depend in great measure on the locality affected, the nature of its epithelium, the amount of elastic tissue and richness in lymphatics. An excellent description is given of the differences in histological features in the mucous membrane of the mouth, nose, pharynx, vocal cords and trachea, and these anatomical differences are made use of to explain many features of the disease. Thus, the lymphatic and vascular systems play the most important part in the production of the severe constitutional symptoms ; hence it is that "the large number and size of, as well as the direct communication of the lymphatic ducts of the Schneiderian membrane with the lymphatic glands of the neck, accounts for the dangerous character of diphtheria of the nose." The scanty blood-supply and the absence of lymphatics explain the mild course of diphtheria of the tonsils. The vocal cords are covered with a pavement epithelium and have very few lymph vessels and no mucous crypts. They are specially prone to be attacked by diphtheria, but the absence of lymphatics prevents the constitutional effects witnessed in affection of the soft palate and nose.

Dr. Jacobi believes in the identity of croup and diphtheria, and his vast experience and sound judgment entitle his opinion to the utmost consideration. Of late years this view has gained ground considerably, and though the distinctions formerly believed to exist between croupous and diphtheritic membranes can no longer be maintained, still the majority of physicians, we believe, hold that the clinical differences between croup and diphtheria are such that they cannot be identical affections. The sporadic, non-contagious character of croup, and the absence of severe constitutional symptoms, are the chief distinctive features. The latter, Dr. Jacobi explains on anatomical grounds, as the vocal cords possess no lymph-vessels, but the affection is not always confined

to the cords, and even when it extends to the trachea and bronchi, in which the lymph-vessels are by no means scanty, we have not the severe systemic infection. The explanation offered of the occurrence of sporadic cases seems rather strained: "Where the poison is insufficient for general infection, it is at times capable of still producing local phenomena. Where an epidemic of diphtheria has died out, a local diphtheritic infection can still take place, and individual cases occur now and then with an almost insignificant power of infection. Such occurrences take place for years or decades, and give rise to the so-called sporadic membranous croup, in the same way as we have for years heard of an occasional case of sporadic cholera, or of a few cases of small-pox. There may not be sufficient infectious material to act on the blood, larynx, or pharynx, but just enough to gain a foothold on the prominent vocal cords with their pavement epithelium." The absence of contagion in these sporadic cases, even under circumstances the most favorable, is very remarkable. We call to mind an instance, in an overcrowded and badly ventilated Infants' Home, of a child with extensive membranous laryngitis and tracheitis, in which isolation was not practised, as the case was believed to be croup, and yet none of the other children (eleven or twelve) in the room took the disease. Dr. Jacobi appears to consider all forms of membranous exudation to be dependent upon the poison of diphtheria, but we incline to the belief, as expressed in the report of the Committee of the Royal Medico-Chirurgical Society, "that there are classes of cases distinct from that disease."

In the exhaustive section on treatment, the author, in the first place, pleads against the expectant method, and urges the prompt and frequent application of the remedies. "Only the philosopher may be a passive spectator, the physician must be a guardian." The use of alcohol at the earliest appearance of general sepsis is strongly recommended; the danger often lies in not giving enough. A remarkable statement is given, illustrating the difference in death rate under the stimulating and depleting methods of treatment. In Rupert, Vermont, up to the year 1862, the latter plan was in vogue, and 90 per cent. of the cases died; since that date, when the mode of treatment was changed, the recoveries have been 90 per cent.

Chlorate of potassium and chlorate of sodium are advised as prophylactics, as they keep the mucous membrane of the mouth in a healthy state. Mild cases of the disease, particularly when confined to the tonsils, do well on chlorate of potassium alone. In the

administration of this drug some useful directions are given ; the dose should be small, not exceeding two grammes in the day for a child two or three years of age, and six or eight grammes for an adult. The solution to be weak and taken often. The dangers of large doses of this salt are very forcibly dwelt upon ; many instances of fatal nephritis are given, which resulted from its indiscriminate employment in diphtheria. Dr. Jacobi even goes so far as to attribute, in some measure, the increase of nephritis during the past twenty years to "the recklessness with which chlorate of potash has become a popular remedy." As the constitutional disease may, in many cases, be compared to the septic absorption in wounds or the puerperal state, the local treatment by disinfection becomes all important. The various substances which have from time to time been brought forward and have met with some success, are fully discussed. Chloride of iron and carbolic acid appear to be the author's favorite remedies ; the former in moderate doses, both for its local and general effects, and the latter locally in a 1-per-cent. solution, and internally in doses of a few grains to half a drachm daily. No good effects followed the use of salicylic acid, either in strong or weak solutions.

Unless nearly detached, membranes must not be removed. In diphtheria of the nose general infection is especially liable to follow, and disinfection, at short intervals, must be kept up.

In the treatment of diphtheria of the larynx—membranous croup—the same general indications must be fulfilled as in the pharyngeal affection. Emetics are serviceable only to remove the mucus and partially detached bits of membrane from the larynx. The sulphates of zinc and copper are preferred. With reference to tracheotomy we had hoped to have had a more favorable account from Dr. Jacobi, but his percentage of recoveries has been so small that, as he says, "only the utter impossibility of witnessing a child's dying from asphyxia has goaded me on to the performance of tracheotomy." The very encouraging statistics from Langenbeck's clinic are quoted, as well as those of Wanscher, of Copenhagen, and Buchanan, of Glasgow.

Of the work, as a whole, it is impossible to speak too highly ; upon every page is evident the fitness of the author for his task. It fully sustains the reputation which Dr. Jacobi has acquired both in Europe and in this country, as one of the foremost clinical physicians of his generation.

[w. o.]

The Surgical Disorders of the Urinary Organs. By REGINALD HARRISON, F.R.C.S. London: J. & A. Churchill, pp. 395.

The critic who examines a series of published lectures delivered to a class of students, must not expect to find a complete and scientific exposition of the subjects under consideration. The relation of the subjects to the time at the command of the lecturer, together with the difference between a scientific elucidation and a concise and practical presentation, necessitates much abridgment, as well as a close adherence to the every-day salient points. The book we are called on to review is composed of thirty lectures delivered by Mr. Reginald Harrison at the Liverpool Royal Infirmary, to which is given the title, "Lectures on the Surgical Disorders of the Urinary Organs." We think the title includes rather more than the text treats of. If the author had christened it "Lectures on many of the Surgical Disorders of the Male Sex," it would have corresponded more nearly to the matter therein contained. It is dedicated, and quite properly, "to the students, past and present, of the Liverpool Royal Infirmary." It is a book evidently intended to enlighten the minds of students, young practitioners and those who, from lack of opportunity, are unable to gain practical information on the subjects of which it treats. The first lecture is devoted to introductory remarks, definitions of stricture, its positions, varieties, etc. It contains many useful hints and practical landmarks which, while not new or unusual, are grouped in an interesting and serviceable manner. The second lecture treats of the surgical anatomy of the urethra, its dimensions, curves, relation to the rectum, fascial attachments, etc. The explanations and illustrations given are stereotyped, even to representing the posterior layer of the deep perineal fascia as being of the same thickness and density as its anterior, a mistake which the much-borrowed illustration from Gray's Anatomy has served to propagate. In our opinion this lecture could be much improved in its anatomical detail; in fact, it should have constituted the first rather than the second lecture of the series; then the reader would have been better prepared to appreciate the relations of stricture to the tissues concerned.

The third lecture treats of the symptoms of stricture. Chapter fourth, on examination of the urine, is a practical exposition of the subject. The rule laid down to first examine the urine before attempting any mechanical interference with the urethra or bladder is a wise one, for if a possible complication is to become a reality,

its preconception will become a source of gratification to the surgeon, and of immense advantage to the patient ; however, there will be deviations from this rule, for obvious reasons. Many amusing and instructive incidents are given, attesting the value of a careful scrutiny of this fluid. It occurs to us that this lecture should have preceded rather than followed the one on symptoms of stricture, since its properties are modified by that condition. Lecture fifth, on the treatment of stricture, etc., opens by giving the usual directions how to pass a catheter, sound, etc. The advice to always use an instrument with a "familiar curve" is in order and should be adhered to.

We believe the "olive-headed," or "bougie à boule," is entitled to a much greater consideration than the author attributes to it. As a means of determining the number, size, situation, etc., of strictures it is superior to the ones figured by the author, but as an agent for dilatation it is of no account. We are pleased to find accorded to the whalebone guides and tunneled instruments the great degree of importance entertained by many American surgeons. The author attaches greater importance to continuous dilatation as a method of treatment than our observation and his associated precautions to such treatment would seem to warrant. We have been accustomed to consider it an exceptional, rather than ordinary method of procedure. The lecture on urethral fever is very practical and well worthy of a careful perusal. We are surprised to find in the lecture on "Retention of Urine" no pointed allusion to the evil effects liable to follow the complete emptying of an over-distended bladder. In lecture sixteen, under head of enlarged prostate, an allusion is made to the evils attendant upon such a procedure. We believe if the author had perused carefully the writings of Gouley, whom he quotes so frequently on other matters, he would have emphasized this important fact under its proper heading. It may be uncharitable to surmise that if the author had been keenly cognizant of its importance he might have avoided the unpleasant complication experienced in the case related on page 117, as well as "on several occasions" before. Watson's steel, probe-pointed catheter is given a prominent place, as a means of treatment "when there is much difficulty in getting through the stricture ;" considering "much difficulty" to be rather an indefinite quantity, often more dependent on the lack of skill of the surgeon than the condition of the stricture, we think this instrument should not be recommended to other than experienced operators. The author does not appear to attach enough importance

to the guides and tunneled instruments, in connection with the class of cases associated with Watson's catheter. Neither time nor space will permit us to consider each lecture in detail. We are happy to be able to say, however, that as the series progresses we find less to criticise. The lecture on enlarged prostate is eminently practical, especially the six points of advice given to those who suffer therefrom. The lecture on formation of calculi is well put together, and contains much practical information. Seven lectures are devoted, principally, to inflammation and calculous diseases of the bladder, and their treatment. Three are devoted to injuries of the bladder, kidney and its appendages. Two to tumors and ulcerations of the bladder. One to circumcision and deformities of the penis, etc. The last one to varicocele. There is much to be said in favor of this book. The lectures are well written, and the ideas forcibly expressed. It can be read with untiring interest. The type is of good size, printing distinct. In fact, the publisher has done his work well. We think, however, that its field of instruction is properly limited to the "present" rather than the "past students of the Liverpool Royal Infirmary," as well as to those who, from lack of time or opportunity to consult more complete works upon these subjects, wish to obtain a fair knowledge of them in the shortest space of time.

[J. D. B.]

A Manual for the Practice of Surgery. By THOMAS BRYANT, F.R.C.S. Third American from the third revised and enlarged English edition, edited and enlarged for the use of the American student and practitioner by John B. Roberts, A.M., M.D., with seven hundred and fifty illustrations. Philadelphia: Henry C. Lea's Son & Co., 1880, pp. 1,005.

The appearance of three English and two American editions of this work within six years was a proof of the estimation in which it is held, and the favor with which it has been received. The author and the publishers recognized the obligation created by this favorable reception, and enlarged and improved each successive edition notably. Much of the original work has been rewritten, and much new material and many illustrations added, bringing the work on most subjects clearly up to date. The book has always been distinguished by its practical character, by the evidences apparent throughout that it has been written by a practical surgeon of large experience, and as such it is especially valuable. At the same time, it is intended to be not simply an expression of

the author's own opinion and practice, but also an epitome of the art and science of surgery at the present time.

The American publishers have now sought in their third edition to adapt it more directly to the American market by having it revised by Dr. Roberts, of Philadelphia. This revision has been wisely confined to the addition of new materials and illustrations, without curtailment or alteration of the original text. The necessity for keeping the book within certain limits of size has caused these additions to be briefer in some cases than we might wish, but they will serve to direct attention to the points mentioned, and often to refer the reader to works where full information can be obtained. The principal additions are the sections on plastic surgery, paracentesis of the pericardium, cholecystotomy, litholapaxy, treatment of fractures, and operations on the neck of the uterus. The last one is but little more than a mention of the operation for lacerated cervix, without directions for performing it or for recognizing the necessity for it. The statement that it is too early to determine the value of rapid lithotrity is rather surprising in view of the experience that has been published, the numerous discussions, and the general agreement of surgeons, not only in America, but also, and especially, in England, France and Germany, upon this point. It is also to be regretted that the note on page 172, referring to the doctrine of cerebral localizations, does not contain any of the facts which may now be considered as definitively acquired, and which have been successfully applied to diagnosis and treatment.

The book is handsomely printed, and the illustrations are above the average. [L. A. S.]

Clinical Lectures on the Physiological Pathology and Treatment of Syphilis, etc. By FESSENDEN N. OTIS, M.D., Clinical Professor of Genito-Urinary Diseases in the College of Physicians and Surgeons, New York. G. P. Putnam's Sons, 1881, 8vo, pp. 126.

After reading the foregoing title, one is somewhat startled to find that Prof. Otis begins with the statement that clinical observation is incapable of settling such scientific questions. Indeed, the work should not have received the title "clinical," for although histories and descriptions of a few cases are introduced, they are used simply to illustrate points brought out in the course of the argument. The whole aim of the work is to indicate a harmony of the course and treatment of syphilis with the author's

views of its pathology. These views were first published ten years ago and are based upon the doctrine of Beale, that "disease germs" consist of degraded cell elements of animal origin, that they are, in fact, the perverted descendants of the ordinary leucocytes of the animal body. Here, these same doctrines are advanced, and the author has brought to their consideration a richer experience and a reasoning strengthened by a keen scrutiny of all recent writings bearing upon the subject. This is discussed under the following headings: 1. History and nature of syphilis. 2. Initiatory period of syphilis. 3. Period of general infection and subsequent localized cell accumulation. 4. Period of lymphatic obstruction. 5. Treatment of syphilis.

As we have remarked, the author asserts that the so-called virus of syphilis does not consist in a specific cell or germ, having no genetic relationship with the protoplasm of the body, but rather that it is the offspring of the normal lymph corpuscles or leucocytes altered by some not well-defined influence, so that it is no longer capable of assuming the higher development that normally belongs to it; that in losing its proper functional activity it acquires an increased reproductive power, such as is the case with these corpuscles when subjected to unfavorable influences, as in the inflammatory process. According to these views, the author accounts for the propagation of syphilis from one individual to another, by the deposition of these particles of degraded protoplasm upon a surface where they are brought into contact with normal lymph cells or white blood corpuscles, which they infect and stimulate to more active reproduction. This process results in a local accumulation of solid elements in the lymph spaces and in the adventitia of the blood-vessels of the part, causing thickening and induration. The thickening of the tunica adventitia, offering an obstacle to the passage of fluid from these vessels, has, as its result, the close packing of corpuscles, which, not being separated by a fluid medium, offers to the fingers the toughness known as the syphilitic induration. Whatever the form of the initial lesion, the foregoing represents, according to Otis, the true "physiology of syphilitic infection." "The syphilitic cell accumulation progresses in all directions until an entrance of the vitiated elements into a lymphatic vessel is effected" (p. 13).

Thus it is that the author unhesitatingly rejects the theory generally adopted, that the chancre is the evidence of a systemic infection effected at the instant of the impure contact. For him, the individual is at first only syphilitic in the small area comprised

within the limits of the initial lesion. As the cell accumulation progresses to a lymphatic vessel, as the vitiated cell activity is extended to the nearest lymphatic ganglia, more and more of his body becomes syphilitic. But he does not have general syphilis until the lymph stream has discharged into the blood current its degraded corpuscular elements, which communicate their perverted molecular motion to all protoplasmic particles with which they are brought into contact. In accordance with these views, Otis rejects the idea of a period of incubation as ordinarily understood, and designates the interval from the date of infection until the passage of the diseased corpuscles into the blood stream, as the "initiator period of syphilis."

Following the natural course of the disease, the period that next ensues, the stage of general manifestations, is termed by the author "the period of general infection and subsequent localized cell accumulation" (secondary syphilis). Roseola, the earliest constitutional symptom of syphilis, Otis does not consider essentially syphilitic, but rather, like simple roseolas, "the result of an impression upon the sympathetic nervous system, a paresis of the vaso-motor nerves of the cutaneous envelope, caused by a special but limited paralyzing influence exerted upon the great sympathetic nerve, through positively recognized blood changes, immediately preceding, accompanying or following an initiation of the so-called secondary or active period of syphilis" (p. 28). It is the herald of syphilitic constitutional infection. The subsequent lesions of this stage of the disease are all essentially syphilitic, and the manner of their production is the same, the differences being of degree only. For example, the pathology of the syphilitic papule will serve as the type of all, and is thus explained. The disease germ, carried by the blood current to the capillaries of the papillæ of the skin, finds its way, by the natural process of exudation or by its own amœboid movements, into the tissues, where proliferation takes place, and results in the dense cell accumulation that characterizes the papule and is identical in nature with the initial lesion. This proliferation is the result of the influence of the disease germ upon the leucocytes. This position Otis supports by citing the views of pathologists upon the anatomy of the lesion. All lesions occurring during the active period of the disease "are dependent solely upon a localized cell accumulation." Where relapses of lesions occur "it seems most probable that this accident is due to the liberation of still active disease cells from isolated points (inferentially lymphatic glands) which have been

for a time separated from the general lymph current by obstructions (in the interior, or of their efferent vessels), from accumulations in the earlier stages of the disease" (p. 40).

With this period, Otis claims that the disease, syphilis, ceases; the patient is no longer syphilitic. Subsequent lesions can no more be called syphilitic than we can consider "dropsy as a stage of scarlet fever, or stricture as a stage of gonorrhœa." The author consequently agrees with those recent writers who look upon the so-called tertiary syphilis as a period of sequelæ. These sequelæ are not "due to a hyperplasia, such as we recognize in the papular syphilide and in all the new formations of the active stage of syphilis," but depend upon "a *deposit*, not of *morbid* but of *arrested normal material*" (p. 45). The gummy products are due to contractions and obstructions of lymphatic canals, the results of lesions that occurred during the "period of localized cell accumulation," in consequence of which the tissues in the neighborhood become infiltrated with normal corpuscular elements, that would otherwise have found their way back into the general circulation. According to this view the author prefers the term "period of lymphatic obstruction," to that of "tertiary" or "gummy period," the "lymphatic obstruction" depending upon the contraction and obliteration of lymphatic channels following lesions that may have existed years before, during the active period. All lesions of whatever character and of whatever tissue occurring during this period are of identical nature.

In the consideration of treatment, Prof. Otis combats the opinions of those who claim for the remedies employed an antidotal action. This is the natural outcome of his views relating to the syphilitic "virus." Syphilis being, in his opinion, not due to a specific "contagium," but rather the result of infection with the vitiated descendants of the white blood or lymph corpuscles, by which there occurs an active accumulation of cells in the tissues of the part first infected, and subsequently in the tissues of the body generally, it follows that the remedies employed for its cure, act by hastening metamorphosis of tissue, by causing fatty degeneration and thus effecting the disappearance of these accumulations. This view is, of course, not novel, but it is a consistent one for him who entertains the pathological opinions we have been discussing; who considers the essence of syphilis to be an "influence and not a material substance."

Beginning with the treatment of the initial lesion (a strictly local affection, following Otis), the recent experiments with the ex-

cision of chancre are noticed and the practice commended, simply as a method of curing the local affection speedily and conveniently, not as a means of averting general infection which, unfortunately, will have already advanced along the nearest lymphatic channels beyond the limits of the excision. For the purpose of "effecting the disintegration and elimination of the cell elements which alone have been recognized as the source of trouble," mercury is the remedy *par excellence*. Remedies, however, capable of causing fatty degeneration and decomposition of *healthy* structure, and so, all the more readily, of degenerated tissues, other than mercury, are of positive value in the treatment of syphilis; iodide of potassium is the most important of these. Mercury, therefore, acts not as an antidote, but by increasing tissue metamorphosis, causes the rapid elimination of the most unstable tissues; consequently, it attacks most energetically and disintegrates the degraded accumulations of the syphilitic process.

The author points out that the true object of the treatment of the active lesions of syphilis is to secure the system against its later manifestations. The earlier ones, within a year or two, subside spontaneously. It is the "period of lymphatic obstruction" that gives syphilis most of its terrors. Prof. Otis concludes that "in all the late lesions due to syphilis" two pathological conditions are to be considered: first, the accumulated material; second, the fibrous material causing the contraction or obliteration of the lymphatic channels. The first condition will yield to the influence of iodine and iodide of potassium. For the second condition these are not sufficient, and they must be supplemented with mercury, if the results attained are to be permanent.

The work concludes with a series of six "class-room lessons," containing a synopsis of the author's teachings upon various questions appertaining to syphilis, as follows: 1. The initial lesion of syphilis. 2. The incubation of syphilis. 3. The contagion of syphilis. 4. Syphilis by hereditary transmission. 5. Early differential diagnosis. 6. Progress of the syphilitic infection.

According to the prevalent theory of syphilis, at the moment of contamination the entire system becomes infected, and there results a period of incubation during which the "virus" is gathering up strength throughout the tissues. At the end of this incubative period, which may last for a few days to several weeks, the

constitutional affection manifests itself, but only at the point of infection. Exhausted, as it were, by this supreme effort, the syphilitic poison again "incubates," and after the lapse of often many weeks, the disease, which has been constitutional all the time, first shows itself as a positively systemic affection, by manifesting general symptoms. This theory is based upon much pure hypothesis and but a single supposed fact. This "fact" is, that when the syphilitic "virus" has once found a point of entry, no earthly power can avert the appearance of general syphilis. Indeed, a number of experiments and observations have been recorded, showing that cauterizations at the point of infection, made immediately after contamination, have been powerless to prevent the subsequent course of the disease. If, now, this "fact" should prove to be no fact at all, the only solid foundation to the theory would be destroyed. Recent experiments in excising chancres (Auspitz and Unna, Kölliker and others) tend to show that a timely removal of the chancre may definitely avert systemic infection; and if future observation confirm the truth of these, as seems quite probable, the theory will have no support left.

On the other hand, the theory adopted by Prof. Otis has no such mystical surroundings, but is based upon a perfectly reasonable, consistent hypothesis, and, whether it be right or wrong, has the merit of being comprehensible from a clinical standpoint and not inconsistent with our present ideas of pathology. That the so-called virus of syphilis depends upon an altered or vitiated condition of the leucocytes is readily conceivable; and one can likewise understand how such altered particles, implanted upon an appropriate surface, may increase by division and contaminate the normal protoplasm with which they come into contact, setting up an active cell accumulation at the point of infection, slowly advancing and finding admission to the lymph current (in passing, however, it may be well to remark that Auspitz and Unna think that the blood-vessels are concerned in taking up the contagium), being arrested for a time in the nearest lymphatic ganglia, finally advancing through these and finding admission to the general circulation, where they soon make themselves manifest. These processes would occupy a certain period in establishing the primary local affection, corresponding to the so-called first period of incubation. The advance along the lymphatics and the temporary arrest in the lymph ganglia would correspond to the "second incubation;" after which the disease would become general. It is not improbable that the pathology of these processes

is as described by Prof. Otis, and it is certain that a consideration of the histology of the initial lesions, so far as we know them, offers no obstacle to his views.

Passing to the secondary period, or, as the author terms it, the period of "general infection and subsequent localized cell accumulation," we find the lesions described as strictly comparable to the initial lesion; they are composed of groups of cells whose degraded life prevents their performing the offices of normal lymph and white blood corpuscles, and which, losing the functional powers for which they were formed, acquire a more active reproductive faculty, which they exercise. But it is among these lesions that we encounter a serious obstacle to the establishment of the theory. Papules, vesicles, pustules, mucous patches we can easily imagine to be "localized cell accumulations," but roseola can hardly be classed as a lesion of this kind. Otis recognizes this difficulty, and avoids it by attributing the process only secondarily to the syphilitic influence, which exerts its direct action upon the vaso-motor nervous system. Syphilitic roseola is "the result of an impression upon the sympathetic nervous system, a paresis of the vaso-motor nerves of the cutaneous envelope, caused by a special but limited paralyzing influence exerted upon the great sympathetic nerve through positively recognized blood changes," etc. It is evident that the necessities of his theory demand that Prof. Otis should exclude roseola from the list of lesions due directly to the syphilis, since it offers no "localized cell accumulation." But it seems certain that this symptom is quite as specific as any other form of syphilitic eruption, and if it be rejected simply because the "localized cell accumulation" is not observed, one may well retort: "Why include with the positive syphilitic manifestations the cell accumulations called papules, since, histologically, they are not unlike papules of whatever origin?" The argument in favor of his opinion, however, is cogent, and if roseola be considered as representing the effects of disordered circulation due to the action of the syphilitic principle upon the vaso-motor centres, it may be classed with the *malaise*, headache, rheumatoid pains, etc., as the effects of the syphilitic infection of the blood itself. The position, that under such circumstances, we have, upon the one hand, the direct influence of syphilis exerted upon the tissues, and, upon the other, the secondary effects of its presence in the circulation, would be tenable.

When he comes to apply his theory to the elucidation of terti-

ary syphilis, or "the period of lymphatic obstruction," the author exposes himself to very serious objections. His views accord with those of many recent writers in considering the syphilitic principle to be exhausted with the secondary period of "active cell accumulation," and in regarding all the subsequent disorders simply as sequelæ. We have seen that Prof. Otis holds these manifestations to be the results of obstructions in the lymph channels impeding or preventing the return of normal cell elements to the general circulation, and the obstructions to be due to contractions following the lesions of the active or secondary period. It "is not due to a hyperplasia such as we recognize in the papular syphilide and in all the new formations of the active stage of syphilis," but "it is due chiefly to a *deposit*, not of *morbid*, but of arrested *normal material*" (p. 45).

When we reflect that these lesions may appear from one to thirty or forty years after the initial lesion, it becomes a little difficult to understand how a fibrosis, such as must, here, necessarily take place, according to our author's views, can, in the longer intervals, require so much time to produce the necessary amount of contraction; and then, having produced it, how it may promptly and definitely be removed with the aid of a little iodide of potassium and mercury. It must be remembered that Prof. Otis denies all specificity in this "period of lymphatic obstruction," so that the process may be strictly compared to any fibrotic contraction resulting from an antecedent hyperplasia, such, indeed, as one finds in the liver, kidney, urethra, and, indeed, anywhere. Occurring near the lymphatic channels, this fibrosis is supposed to occlude these vessels and cause the damming up of corpuscular elements. One does not expect to cure fibrosis easily and speedily with iodide of potassium and mercury, when it occurs in any other situation; nor is it reasonable to expect its disappearance when the contraction follows a syphilitic hyperplasia. Whatever the cause of the original cell accumulation, the resulting formation of contracting fibrous tissue is the same. Obstruction from contraction of these lymphatic canals is, however, a pure assumption and has never been demonstrated. The case cited from Biesiadehi has no definite bearing upon this point, since it is not improbable that the lymphatic distension followed and did not precede the gummy ulceration. In those affections where we have every reason to believe obstruction of lymphatic vessels does occur, such as elephantiasis, the symptoms are quite different from those of gummy tumor.

The author lays great stress upon the fact that the microscopic appearances of gumma offer nothing characteristic, and must consequently have no specific origin. As well might he say the same of tubercle and sarcoma, since frequently both of these new growths are indistinguishable from gumma; and yet we know that these three varieties have no clinical relationship; on the contrary, are governed by positively different nutritive influences. Although sarcoma and tubercle probably do not depend upon a "disease germ," some special influence presides over their development, and for the gummy infiltrations of syphilis an equally specific influence may be assumed. That this influence is different from that exerted during the active stages of the disease is granted, but where an affection is characterized by certain peculiarities of history, of development and of termination, it follows that there must be something special in its nature. At all events, that it is a simple cell accumulation from obstructed lymph channels is disproven by the anatomy and history of the disease as well as by the results of treatment.

Dr. Otis' doctrine of the influence of remedies over the course of syphilis is based, naturally, upon his theories of the nature of the disease. Consequently, he rejects the antidotal influence of anti-syphilitic remedies, preferring to regard these agents as promoters of disintegration and elimination of tissues. This is undoubtedly a consistent and philosophical explanation of the action of these drugs, and has been already adopted by a number of observers. A single reservation must be made, however, in regard to the obstruction of lymph channels following active lesions. If the author's views of pathology are correct, these contractions (as we have seen) must be after the nature of a fibrosis, which mercury and iodide of potassium cannot modify. If it be asserted that the obstructed corpuscular accumulations only are removed by the disintegrating influence of these remedies, then, without fail, similar corpuscular heaps would re-accumulate as soon as their use was abandoned. Clinically, we know, this is by no means the constant result.

The theory advocated by Prof. Otis may be considered under two divisions: the first including its application during the active stages of syphilis, or, as is generally understood, until the end of the secondary period; the second including the period of "lymphatic obstruction." So far as concerns the efficient cause of syphilis and its manner of affecting the entire organism through the lymphatic system, and the general pathological relations of the

active stage of the disease, his views are consistent, scientific, and in accordance with what we know of pathology. Moreover, they offer a rational explanation of processes, such as no other theory at the present time affords. If now, instead of accounting for the "period of lymphatic obstruction" by the cicatricial obliteration of lymphatic channels from pre-existing specific hyperplasia, the author had still claimed a specificity for the processes, not in the blood, but in the tissues, or rather in certain localities in the tissues, modified by time, by long quiescence, or by influences that as yet escape recognition, he might still urge his theory with perfect consistency; but, in claiming that syphilis ends with "the period of active cell accumulation," and that the latter manifestations are perfectly simple in nature and due to mechanical obstruction, he places himself at variance with clinical observation and therapeutic experience. The influence of iodide of potassium is but feebly exhibited over simple inflammations and ordinary lymphatic obstructions. If its power over late syphilitic lesions is so marvellous, it must be because these differ from simple hyperplasie and new growths.

[I. E. A.]

A Treatise on the Diseases of the Eye. By J. SOELBERG WELLS, F.R.C.S. Third American from third English edition, with copious additions by C. S. BULL, A.M., M.D.

It is a source of surprise that a standard text-book like the above should have gone out of print in less than five years after the appearance of its last edition in 1873, and the reason is probably that its gifted author found no leisure to revise it, while the rapid appearance of newer works by distinguished men left it in the background, by reason of the advance of ophthalmology during the past decade. While we join the American editor in regretting that the revision does not come from the hand of the lamented author, we congratulate ourselves that the task has fallen to one who is in every way so capable of fulfilling it, one whose contributions to ophthalmic literature are rapidly raising him to the position of an authority.

Ophthalmic science is so firmly established as a special branch, its devotees are so many and its literature so extensive, that its mooted questions are not fought over with such heat and excitement as newer specialties, like gynecology, are prone to exhibit. There is an established method of treatment for all conditions, sanctioned by high authority and tested by time, and any change is an innovation which must prove its right to

exist by challenging and vanquishing the old plan. Recognizing this, and remembering that a text-book should always lean rather to the side of conservatism, the editor has refrained from passing judgment upon doubtful points, but has brought them forward in their proper places and made copious references to articles where those interested may find full discussion of the subject.

In the nomenclature of the different diseases the editor frequently introduces synonyms which might well be adopted as substitutes, such as "conjunctivitis" for "ophthalmia," "superficial keratitis" for "pannus," etc.

In the introductory chapter we are glad to find mention of Carmalt's perimeter, since, although a much cheaper instrument than its predecessors, we have found it equally serviceable, and think it will tend to make careful measurement of the field of vision more frequent. The chapter on diseases of the conjunctiva includes a description of Buller's ingenious device for protecting a sound eye from contagion from its fellow, which Dr. Buller has assured us is so perfect that he does not hesitate to inoculate with purulent matter for the cure of vascular keratitis, while the other eye is perfectly sound. Membranous or croupous conjunctivitis is given a separate mention, though considered by the editor not to be a distinct variety, in which opinion we think he would find many opponents on this side the Atlantic as well as on the other. The influence of quinine applied locally to restrain suppuration of the conjunctiva, mentioned as supported by Bader and Nagel, ought to be more thoroughly investigated, for any useful addition to our stock of weapons against chronic conjunctivitis would be welcome. The ingenious operations for transplantation of conjunctiva without pedicle from the human subject or from the rabbit, made by Wolfe and others, can only be alluded to in a work of this kind, and we believe that several interesting observations on that subject by Noyes, of New York, have not yet found their way into print. The advances in the department of ophthalmic pathology are shown by the new matter in the section on tumors of the conjunctiva, subconjunctival cysts, sarcomata, primary carcinoma, medullary cancer and papillary fibromata being among the innovations. Syphilis, lupus, lepra, amyloid disease and tuberculosis, as affections of the conjunctiva, also receive their first notice in this edition. Chapter II, on the cornea, alludes to duboisia as a substitute for atropia, but considers its place in therapeutics unsettled as yet. Eserine and pilo-

carpine are damned with faint praise, which, in our experience as regards the former, is unmerited.

In the treatment of dense opacities of the cornea by the novel method of transplantation of a piece of healthy cornea into the gap left by removing a section of the opaque one, mention is made of the operations of Wolfe and Sellerbeck. We would add a reference to the exhaustive experiments of Neelsen and Angelucci, as well as of Zehender in the *Klinische Monatsbl.* for 1880.

Chapter III, on diseases of the iris, contains an excellent description of the rare form of iritis known as "gelatinous" or "spongy." Lepra and sarcoma as a primary affection of the iris are brought forward, though the former especially is an extremely rare disease. The operations of iridodesis and iridenkleisis are very properly condemned, for, to the best of our knowledge, they are abandoned by those who formerly thought well of them, since they have been found to excite cyclitis and even sympathetic disease. In Chapter IV, optico-ciliary neurotomy, or division of the optic and ciliary nerves without removing the eyeball, as a substitute for enucleation, is held to be still *sub judice*, and the editor contents himself with a description of the operation, not expressing any opinion as to its sufficiency; in this, as elsewhere, exhibiting a careful conservatism.

Chapter V contains a timely reference by the editor to those lenticular opacities, which are not so infrequent as is supposed by some, and which are not incorrectly diagnosed as "cataract," but which, having no progressive tendency, have not the prognosis of ordinary cataract, and probably are the kind of cataracts so frequently "cured" by enthusiastic therapeutists. The editor's conservatism does not prevent him from expressing an entire disbelief in the cure of cataract by electricity, in which view all reputable ophthalmologists join. The "antiseptic method" of operating for cataract has received a fair trial at the hands of the German surgeons, but presents difficulties not encountered in other regions of the body. The editor states, and we think correctly, that it has met with little favor in the United States. We are glad to see the statement, which we believe to be correct, that Dr. Gruening, of New York, was the first to suggest and employ the peripheric section of the capsule, for our trans-Atlantic friends assign it to another surgeon.

Chapter VI, on the use of the ophthalmoscope, brings forward the American modifications of the instrument, bearing the names of Knapp and Loring, which bring the ophthalmoscope so much

nearer perfection than any form of foreign derivation. The Loring instrument with Wadsworth's tilting mirror, judging from our own experience with it, leaves little to be desired in this direction, and the extract from Loring's monograph on "Determination of the Refraction of the Eye with the Ophthalmoscope" is so plainly worded that the veriest tyro ought to comprehend it. We are glad to see Loring's explanation of the halo around the macula, first published in 1871, for although so sharply criticised by German ophthalmologists, who seem to have lost their tempers in the discussion, it is the most reasonable theory thus far offered in explanation.

Chapter VIII, on retinal diseases, gives the editor an opportunity to correct the too prevalent idea promulgated by Liebreich, and endorsed by the author, that the appearances so frequently seen in Bright's disease are pathognomonic of that condition, and to say that "the presence of Bright's disease of the kidney cannot be diagnosed with certainty by means of the ophthalmoscope alone." The teachings of Bouchut, Hughlings Jackson and others, have tended to cast discredit on ophthalmoscopy by claiming for it powers which it does not and probably never will possess, especially in relation to intracranial conditions which now seem to be daily diagnosed by neurologists who are mere tyros in ophthalmoscopy, and who make fine diagnoses of "hyperæmia of the retina" with an ease and abandon which would be laughable were it not for its reflex influence upon true ophthalmoscopic science. In this connection we would call attention to the editor's remarks on "choked disc" in the following chapter where, while admitting that brain tumors are the most frequent cause of the phenomenon, he says that either may exist independently of the other, and that the situation of a brain tumor cannot be determined with any accuracy from the presence of optic neuritis. Those interested in this connection between ophthalmoscopy and general medicine would do well to read an article by Dr. Carmalt in the *Trans. of the Conn. Med. Soc.* for 1880. The advances recently made in the problem of the decussation of the nerve fibres in the chiasm are well and succinctly set forth, and reference made to exhaustive papers on the subject, but the whole subject is in an unsettled condition, and as such cannot be epitomized for a text-book. Strychnine injections in the treatment of amblyopic affections are recommended in cases where no retinal lesions are evident, and are not discountenanced even in cases of true nerve atrophy—though in this field the editor, in common with most ophthalmologists, has

not met with the marked success of Dr. Webster, given in a paper read before the N. Y. County Med. Soc., subsequently to the issue of this edition. Perhaps Dr. Webster's success is due to his boldness in the use of the drug, for he does not hesitate to increase the dose gradually to the $\frac{1}{2}$ of a grain daily, if he does not get the physiological effects manifested under less amounts. We have got marked effects in a few cases under this plan of treatment.

Color-blindness is a subject which has developed so much and been so prominently investigated by some of the master-minds of the profession, since the previous edition, that it appears like a new theme. The researches of Holmgren, Donders, Stilling and Jeffries are referred to, and serve to set the subject forth in the light of a practical scientific problem, rather than, as heretofore, of a scientific curiosity. The comparative frequency of the affection among males, viz., 4 per cent., and the universal use of red and green signals in railroad and steamship service, have already led to official examinations for this defect, both here and in European countries.

Chapter XII, on glaucoma, introduces a subject about which our German brethren have been specially concerned, and the two great theories as to its nature—the inflammatory and neurotic—have each their adherents. A short *résumé* of the most important papers by Knies, Weber, Schnabel, Mauthner and others, sets forth the main points of the argument, and will be of service to those who have not the opportunity to read the original articles. De Wecker's operation of incision of the sclerotic, "sclerotomy," as a substitute for the trusted iridectomy of von Graefe, is described, and Mauthner's rules for its indications are given, but the subject is still unsettled, and glaucoma in America is so comparatively rare, that the decision of the question is not likely to come from this side the ocean. The use of eserine in glaucoma is mentioned but without comment.

Chapter XIII, on refraction and accommodation, brings forward Dr. Thomson's ingenious instruments for determining rapidly errors of refraction. We have not found the perforated discs of much service, unless the patient were thoroughly intelligent; but the "ametrometer," described in the *Trans. Am. Oph. Soc.*, 1878, we have found a very useful and time-saving appliance. The editor seems to have mixed the descriptions of the two latter instruments in a fashion that might prove puzzling to a novice.

Chapter XIV, on affections of the muscles, contains nothing

new, except the operation of Dr. Noyes, of Detroit, for the cure of strabismus, which consists essentially in advancing the antagonistic muscle in place of setting back the one now usually operated on. This operation, though brought forward in 1874, is not, we believe, done to any extent except by its originator.

Chapter XV, on diseases of the lachrymal apparatus, commends the destruction of the sac, where indicated, by an incision through the conjunctival cul-de-sac, instead of through the skin. This method, which we have always credited to Dr. Agnew, of New York, we have seen him do several times, and regard as a great improvement. The use of the "dental engine," in the removal of ivory exostoses of the orbit, will probably be accepted by American, sooner than European surgeons, as the instrument has been considerably used in bone surgery on this side of the Atlantic.

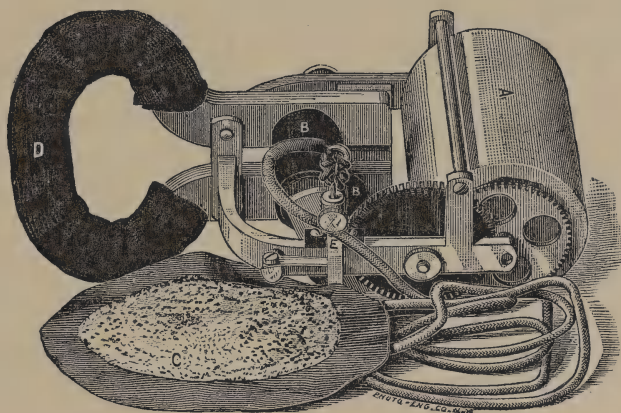
In Chapter XVII, on diseases of the eyelids, it is evident, from the variety of operations detailed for the cure of entropium, that we have not yet hit upon the ideal one. Among the new ones here may be found those of Hotz, Green and Schneller.

The mechanical execution of the book is very good, and the new illustrations, of which there are many, add greatly to the effectiveness of the work. We congratulate the editor upon having done his work so well, and the medical public in having so valuable a text-book brought down to date. [S. B. ST. J.]

Instrument for Electro-massage.—Dr. John Butler, of this city, has introduced to the profession a new and very ingenious instrument, in which a roller electrode is made to cause rotation of a pair of helices near a magnet, and thus give rise to an induced current of sufficient strength to produce muscular contractions. The principle of production of the current is the same as in ordinary magneto-faradic machines, the invention consisting in utilizing the power to do massage at the same time that current is produced. In the improved machine which we have examined the magnet is stronger than in others, and the roller is fully three inches broad. The absolute utility of this contrivance remains to be determined by experience. The notion of combining mild faradization with mechanical excitation of the muscles is a good one, and has been carried into practice before by means of roller electrodes connected in the ordinary way with a battery.

In Dr. Butler's instrument, as in other magneto-faradic instruments, we find the current jerky and painful; much less accept-

able than that coming from a well-made volta-faradic machine. The effect of pressing upon the muscles with the roller is by no means the same as intelligently-done massage by the fingers, but, except in large cities, this latter treatment cannot be obtained,



and even in New York there are few manipulators who can be trusted to do it well.

Taking all into consideration, it seems very probable that this instrument for electro-massage will meet a need in practical therapeutics.

[E. C. S.]

ORIGINAL OBSERVATIONS.

TRAUMATIC ABSCESS OF THE BRAIN—PARALYSIS OF THE EXTENSORS OF THE WRIST—TREPHINING—DEATH.

By LEWIS A. STIMSON, M.D.

The following case is offered as a contribution to the study of cerebral localizations and the therapeutical indications that may arise from them.

Patrick K., æt. 26, was pounded with a stone upon the right side of the head about May 1, 1880, receiving a scalp wound above the ear. He was taken to St. Vincent's Hospital, where the wound was enlarged and a fracture sought for, but none found. He remained in the hospital a fortnight, and was discharged at his own request.

June 3d he entered the Second Medical Division of Bellevue Hospital, complaining of severe and constant headache. He was dull and listless, his pupils normal and responsive, the extensors of his left wrist and fingers paralyzed. He gave a syphilitic history; his scalp wound had healed. He improved at first on large doses of the iodide of potassium, but soon relapsed, and was transferred to the Second Surgical Division, June 19th, where he rallied and again relapsed before July.

Early in July he came under my care. He was then in a condition of hebetude, did not respond to questions, eyes open, pupils large and equal. There was a scar two inches long on the right side of the head, running upward and forward from a point just above and behind the ear; the adjoining skin was normal; temperature 99°. I inquired if any paralysis existed, but unfortunately that symptom had not been included in the record.

July 15th, while passing his bed, I noticed that he had brightened up again, and remembering that the scar lay close to

the motor centre for the upper limb, I asked him to raise his left arm, and then detected the wrist-drop. I made the diagnosis of cerebral abscess under the upper end of the scar, and called a consultation of the surgeons.

The next day the patient again became stupid, and on the 20th, the day set for the operation, he lay with his eyes open, a quick pulse, temperature 99° , and a bluish look about the face. He answered no questions, and was only slightly sensitive to pain. During July his temperature had risen above 99° only four times, and above 100° only once, July 10th, when it rose to $102\frac{1}{2}^{\circ}$.

Operation.—Head shaved. No anæsthetic. The Rolandic line was marked and found to pass about half an inch in front of the upper end of the scar.* A vertical incision, two inches long, its centre at the upper end of the scar, was then made. The knife, as it crossed the scar, struck a piece of loose bone, evidently a portion of the external table, $\frac{1}{2}$ inch long and $\frac{1}{8}$ inch wide, which, when removed, disclosed a small perforation of the skull filled with granulations. There was no pus about it. The pericranium was loosely adherent, and a shallow, narrow groove ran along the surface of the bone, from the opening, for nearly an inch, in the direction of the scar. The trephine was applied on the anterior side of the opening, the disc removed, and the edges explored. No trace of fracture of the internal table could be



FIG. 1.

B, the Rolandic line; *A*, the scar; *C*, the point at which the trephine was applied.†

* The patient's head was exceptionally high and short, and the nostrils lay well below the ear.

† The lower end of the line *B* is too far forward. The lower end of the Rolandic line is at a point seven centimetres behind and three centimetres above the external angular process of the frontal bone. Its upper posterior extremity is five and a half centimetres behind the junction of the sagittal and coronal sutures.

discovered ; the dura mater rose slightly into the opening, and was pulseless. I introduced a hypodermic needle and drew out pus, then divided the dura mater, cut into the cerebral tissue, and opened an abscess one-quarter of an inch below the surface of the brain, which discharged a quantity of pus estimated at about two ounces. A probe could be passed two inches. Antiseptic dressings.

The patient's temperature an hour after the operation was 102° . His condition remained unchanged, and he died nine hours after the operation.

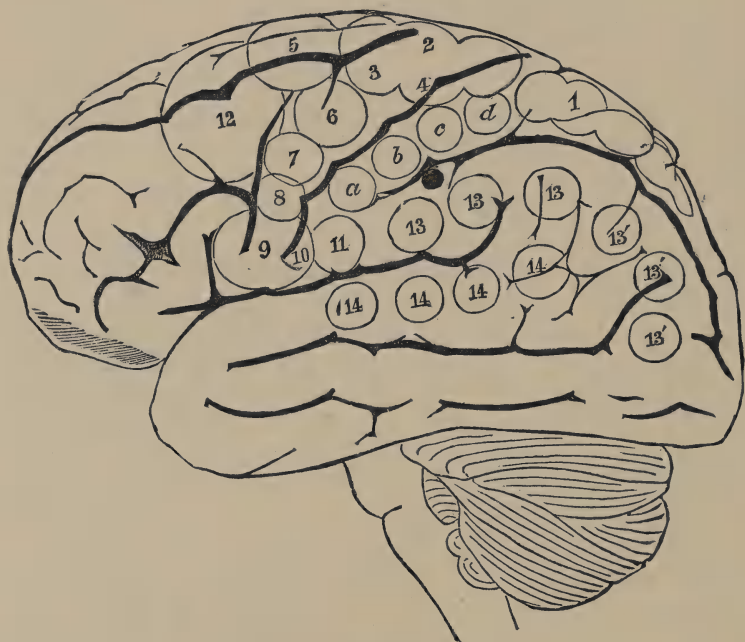


FIG. 2.

a, b, c, d, Ferrier's centres for hand and wrist. The black spot behind them indicates, approximately, the site of the abscess.

Autopsy.—Considerable injection of the pia, but no signs of chronic meningitis, except at the point of injury, where the dura and pia were adherent to each other. The skull at the seat of injury was thinner than usual ; no fracture of the inner table, and no change in the under surface corresponding to the groove in the outer surface. Dura mater normally adherent to the skull and slightly thickened at the seat of injury. The cavity of the abscess

was entirely obliterated, its walls slightly yellow, no signs of any extravasation of blood. The opening of the abscess was in the parietal lobe behind the lower part of the ascending parietal convolution, from which it was separated by a shallow sulcus. Its position is indicated in the annexed figure, which shows also its relations to the centres for the hand and wrist, as described by Ferrier.

TWO CASES OF CHOREA POSSIBLY DEPENDENT UPON MALARIA.

By HENRY N. HEINEMAN, M.D.,

VISITING PHYSICIAN TO MT. SINAI HOSPITAL, AND OUT-PATIENT DEPARTMENT, N. Y. HOSPITAL.

The two cases here related may serve to emphasize the possible relation which is yet to be proven to exist between malaria and chorea.

A. J., æt. 8, was brought to the New York Hospital, Out-Patient Department, for the first time May 20, 1879, with the following history : Father healthy; mother had had choreic movements and impediment in speech during her first pregnancy, seven years previous to birth of this the fourth child. Mother has had no recurrence of this attack, and the other living children are well. Patient has always been well except for an attack of scarlatina at one year of age. Three weeks ago he first developed choreic movements and occasional difficulty of articulation. He was ordered citrate of iron and quinine, and Fowler's solution. At the end of a fortnight he passed from my observation, and I did not again hear from him until May 11, 1880, just a year later. The father stated that the previous spring he continued giving him the medicine of his own accord, his business preventing his calling to see me for advice, and that finally the boy completely recovered and had been in most excellent health up to one week ago, when the choreic movements returned. Upon examination the lad looks healthy and well-nourished; the left extremities are in constant agitation, those of the right side being only slightly involved; speech is thick and articulation difficult. Is unable to hold things in his left hand, and only with difficulty in his right. Is constantly moving about. The return of the lad at the same season of the year awoke a suspicion in my mind that possibly malaria was the cause of his illness. I ordered quin. sulph., grs. iii, *t. i. d.* Ten days later the father reported very slight improvement, but stated

that he noticed that the movements were more marked on some days. A few days later, at my request, the father brought the following report of his daily condition :

May 28th, boy is quiet ; 29th, restless, tongue is decidedly thick and speech seriously affected ; 30th, unsteady, feverish, peevish ; 31st, very quiet to-day ; June 1st, unsteady and weak on his legs ; 2d, very quiet to-day.

I now ordered pills of *

Quin. sulph., grs. ij.
Pulv. capsici, gr. ss.
Pulv. zingib., gr. i.

℥

From five to six daily to be given so as to anticipate his period of restlessness. The diary of the father continues as follows : June 3d, unsteady, nervous, trembling, twitchings ; 4th, noticed nothing about him ; 5th, slight movements of left arm ; 6th, noticed nothing ; 7th, movements and twitchings of the whole body ; 8th, very quiet ; 9th, the same.

From this date all the symptoms ceased, except on one or two occasions, during the following week, slight tremulousness of the legs after an exhausting run or play. The recovery was permanent, which was recognized when he presented himself for inspection in September, 1880.

Recently, February, 1881, the lad presented himself for a third time, with the same history of having been well all the year, and was now again suffering as before. He was feverish at times and had enlarged area of splenic dulness. He was placed upon the same treatment as above, and within a week the symptoms rapidly abated and will have disappeared by the time this report will appear.

The second case was a girl *æt.* 10, a resident of the outskirts of Brooklyn, whom I first saw March 13, 1880. No family history of nervous disease. Had always been healthy, up to four years ago, when, after suffering from measles and diphtheria, she had an attack of chorea lasting some time. In the March following it returned and continued to July, and this has been repeated three successive years. During the remainder of the year, mother states, she attends school, is bright and in perfect health. Mother also says that at first the left side alone was affected, but that since, both sides are affected, though the left side is always worse.

* Formula of Prof. Wm. H. Thomson.

Now, is constantly restless, hand and fingers twitching, moves her legs and head almost constantly. Cannot hold anything with left hand.

The patient was ordered quinine simply, and subsequently citrate of iron and quinine, and rapidly recovered, the date of her last visit to me being May 1st. She has not since returned.

In both the above cases, the presumption of a malarial origin is, I think, quite strong, the first case especially affording me abundant and repeated opportunity for its study. Still further, in both instances, the parent was intelligent and observant and the previous history therefore reliable; both children at the time of their presentation did not appear to suffer from anæmia, and were free from all organic disease.

A CASE OF MALARIAL NEURALGIA.

By WALTER MENDELSON, M.D.,

HOUSE PHYSICIAN TO THE NEW YORK HOSPITAL.

J. F., aged 44, an Austrian, married, and a turner by trade, was admitted to the New York Hospital October 7, 1880.* He gave the following history:

He had lived in this city but a few months, having come here from London, where he had been established for many years. He had never had malarial fever, nor syphilis.

For four or five weeks previous to admission he had been seized nearly every evening, about seven o'clock, with a most intense and agonizing shooting pain which, beginning at the external condyle of the left humerus, extended to the shoulder. The pain would last three to four hours. Occasionally a similar pain appeared at the external malleolus of the left ankle and extended up the limb.

Six days before admission, while working at his lathe, the patient was suddenly seized at 4 P.M. with a chill, and with such an overpowering weakness as to oblige him to sit down. At the same time there was a most severe shooting pain all down the left side of the body, and intense frontal and vertical pain on both sides of the head. The patient thought that he lost his consciousness.

The latter pain existed on admission, the former having gradually worn away. The pain in the head was likened to the sensa-

* Service of Dr. James W. McLane.

tion that might be produced by a knife or red-hot iron being passed through the brain.

The patient had been obliged to keep his bed since the beginning of the attack.

On admission he was extremely prostrated, and seemed to be suffering intensely from the pain in the head. Examination showed great hyperæsthesia of the skin of the frontal and vertical portions of the head, and tenderness on pressure over both the supra-orbital notches and along the course of the supra-orbital nerves. There was no loss of power or of the reflexes in any of the limbs. The pupils were normal and reacted well. The heart and lungs were normal. The rectal temperature was 40.6° (105° F.).

The patient was ordered of morph. sulph. gr. $\frac{1}{8}$ by the mouth, and of potassium bromide 15 grains every two hours.

On the following morning, the temperature having fallen to the normal, a malarial element was suspected, and twenty grains of sulphate of quinine were administered at once, and orders were given to give five grains three times during the rest of the day. The bromide of potash was stopped. The pain and hyperæsthesia were much less. (It might be well to note here that the account of the daily periodic neuralgic attacks in the left arm was not obtained till the patient was nearly well—a week after admission.) During the day, without any previous chill, the temperature gradually rose, being 38.2° (100.3° F.) at 8 P.M.

October 9th. This morning at 6 o'clock the temperature was 39.2° (102.6° F.), when twenty grains of quinine were again given.

With the exception of a profuse diarrhœa, the result, perhaps, of the large doses of quinine, which was easily checked, the patient did very well, the pain gradually subsiding. There was no rise of temperature after the third day following admission, and by the seventh day the pain had entirely disappeared.

On October 18th he was discharged cured. The patient has called to see me several times since his discharge, and says that he has had no return of his neuralgia.

The case is interesting from the suddenness of the onset of the neuralgic pains, their terrible severity (enough to make the man think he lost consciousness), and the absence of any preceding symptoms of typical malarial fever. The periodic pains in the left arm, recurring nearly every day at the same hour for several weeks, and the prompt relief afforded the patient by quinine, would leave but little doubt, however, as to the malarial origin of the neuralgia.

Explanation of the Figures.

Fig. 1. Section of small vegetation on wall of ventricle, Case 1. (*a*) Heart muscle; (*b*) small additional pedicle, in which there is considerable endocardial proliferation; (*c*) accumulations of leucocytes; (*a*) locality where the micrococcus balls are most abundant. (Hartnack, No. 4.)

Fig. 2. Section of the outer edge of a small vegetation showing the fibrillated appearance and the micrococcus balls *in situ*.

Fig. 3. Outer margin of an encrusting vegetation on a chord. There is a nipple-like projection into which the coccus balls have collected.

Fig. 4. Closely aggregated coccus balls, of all sizes, from vegetation on mitral. (Hartnack, No. 7.)

Fig. 5. The same, imbedded in a homogeneous fibrinous matrix. (Hartnack, No. 7.)

Fig. 6. Large coccus ball, with secondary ones in the interior. (Hartnack, No. 9.) All of these are from Case 1.

Fig. 7. Section of vegetation on aorta in Case 5. (Hartnack, No. 4.)

ARCHIVES OF MEDICINE.

Original Articles.

CROUPOUS PNEUMONIA, AN ACUTE INFECTIOUS DISEASE.*

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A CUTE lobar pneumonia or, as the Germans have it, croupous pneumonia, is a disease which, since the time of the earlier writers, has, in one way or another, engaged the attention of medical men as being one of the most important and fatal maladies to which human flesh is heir. Like all diseases of such manifest importance it has been the subject of much study, discussion, and theorizing; and, as might be supposed, many theories have been advanced to account for its various manifestations and phases, most of which have shared a common fate, having, after holding their sway for but a brief period, been cast aside as inapplicable. Two theories now engage the attention of the profession: one long accepted as the true one, regarding the disease as a pure inflammation or phlegmasia; the other, although not of recent date yet comparatively unknown and by most not received, regarding the form of pneumonia we are dealing with as an acute infectious disease, an essential fever, dependent upon a specific blood poison. If one is true the other must necessarily be false.

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As our learned friends the mathematicians have it, all angles are either right or not right ; or, as we may put it, all theories are right or not right, a theory is true or false.

Before proceeding further I would wish just here to emphasize the fact that in the course of this article I am discussing solely and entirely what we now know as croupous pneumonia, catarrhal or broncho-pneumonia being left entirely out of the question. This statement I find necessary, since in discussing the subject on a previous occasion I found several who confounded these two forms of pneumonia, and hence were unable to argue intelligently on the matter in point.

If we have an old, time-worn house, useless and tumbling down, standing on ground whereon we wish to erect a new and strong structure, we would not proceed to build upon the old, but rather would pull down the useless to make way for the useful. Or to put it in a poetical way, you would deem it "best to be off with the old love before you are on with the new." Now, the local theory may be considered our old love, while we have for our new, at least I offer it to you as such, the constitutional or infective theory. Therefore, in accordance with this plan, I shall first attempt to disprove or overthrow the commonly accepted doctrine, before venturing on the positive deduction of arguments designed to establish the infectious nature of the disease.

A theory to command or rather to be deserving of attention must adapt itself to and be applicable to all the various phases and circumstances of the disease to which it is applied. This, I claim, the phlogistic theory of pneumonia does not do, and for the following reasons :

1. Experimentally we can induce a simple inflammation in any part of the body ; we can produce a pleuritis by injury of the pleura, either by means of wounding or by injection of irritant substances ; we can bring about a hepatitis,

a splenitis, a nephritis, or a cerebritis in the same way. All experimentation, whose object has been the production of a croupous pneumonia, has utterly and signally failed. Now, it is a well-known fact that the same result has been reached in attempts at the artificial production of all the diseases belonging to the infectious group. Gendrin (*Hist. anat. des inflammations*, ii, p. 30) found that in an animal made to breathe chlorine gas the lungs were studded with little solid nodules, due to an exudation into the air-cells, a lobular not a lobar or croupous pneumonia. Cruveilhier produced similar results by the injection of mercury into the trachea. Reitz (*Sitzb. K. K. Akad. zu Wien*, 1867; *Math. Anat. Wissch. Cl.*, 2 v., 3) experimented by injecting caustic ammonia into the trachea, an experiment which has been repeated by Wilson Fox (*Reynold's System of Medicine*, vol. iii, p. 616). The results obtained were as follows: Intensely developed membranous exudation, extending through trachea and smaller bronchi, but becoming more fluid and puriform in the latter. No uniform lobar consolidation, but scattered, small, solid, irregular spots, finely granular on section, breaking down in various parts into cavities filled with puriform matter; surely not a croupous pneumonia. Bayer (*Ueber die Versuche croupöse Entzündungen der Respirationsorgane künstl. zu erzeugen*, etc., *Arch. f. Heilkunde*, Bd. ix, s. 85) arrived at the same results after similar experiments. The latest attempts in this direction have been by Bernhard Heidenhain (*Virchow's Archiv*, Bd. lxx). These experiments were made in the Pathological Institute of Breslau, under the guidance of Prof. Cohnheim. Surely, therefore, they are worthy of reception as authoritative statements of facts. The animals were made to breathe hot air through a canula introduced into the trachea. The experiments lasted from 15 to 30 minutes or more. They were killed in 3, 5, or 7 days, and

in every instance the lungs were found perfectly healthy. In other cases the animal was made to breathe ice-cold air, but the results were again negative. The same followed alternate inhalation of heated and cooled air for a quarter of an hour each. The inhalation of moist air at a temperature of 54.4° C. (130° F.) gave rise to a condition resembling catarrhal pneumonia. The vapor of acetic acid produced similar results to those of hot moist air. He concludes, from his researches, that true croupous pneumonia cannot be excited by irritation of the respiratory passages.

Squire (*The Practitioner*, vol. xx, 1878) makes the statement that Friedländer has been enabled to produce pneumonia, presumably meaning croupous pneumonia, by section of the vagi. I quote from his article: "The argument in favor of a specific poison drawn from the difficulty of inducing the disease by direct irritation of the lung is met by the fact, demonstrated by Friedländer, that the disease is readily induced after section of the vagus or recurrent nerve." This statement, to say the least, I find inaccurate. Dr. Wm. H. Welch, of this city, informs me positively that the pneumonia following division of the pneumogastrics is always catarrhal in its nature. This assertion he makes as the result of his own experiments, and also on the ground that he has in his possession several specimens obtained from animals experimented on by Friedländer himself, which show the process to have been undoubtedly a catarrhal one. Buhl considers the pneumonia resulting from this experiment as of the hypostatic form, while Traube and Claude Bernard were of the opinion that the principal cause of this pulmonary inflammation was a paralysis of the œsophagus, with escape of the regurgitating material from the stomach into the respiratory passages, and consequent hepatization; that is, the production of a catarrhal pneumonia by plugging of the bronchial ramifica-

tions, atelectasis, etc., the same as occurs in this disease under ordinary circumstances. Steiner attempted, apparently with success, to prove the correctness of this theory. But to settle this question positively, I quote the following description of the pneumonia following section of the recurrent or vagi nerves, as given by the experimenter himself (Carl Friedländer, *Virchow's Archiv*, Bd. lxxviii, s. 344):

"1. The disease begins with hyperæmia and œdema, which either disappear or go on—

"2. To red hepatization. This red hepatization may still vanish, or, contrarily, two further processes (Nos. 3 and 4) may result, namely:

"3. Desquamative transparent-gray hepatization. This begins, as a rule, within the first two weeks, and may, later on, pass on to resolution, or may lead to atelectasis, but often for months remains stationary. It does not pass on to caseation.

"4. Small-celled, whitish-gray hepatization. This begins to develop in about ten hours, spreading, as a rule, during the first few days. . It is also capable of resolution, but if it commences in a moderately intense form it usually goes on to caseation. In the early period it is a red hepatization, which later on becomes a desquamative hepatization."

Dreschfeld (*Lancet*, January 8, 1876) has also found, on division of the vagi, that a catarrhal pneumonia follows.

It will thus be seen that Dr. Squire is hardly justified in urging these experiments of Friedländer's as opposed to the infectious theory of pneumonia. Therefore, we are forced to conclude that croupous pneumonia has not been and probably cannot be experimentally produced, and that in this respect it differs diametrically from purely local phlegmasiæ.

2. Many causes have been advanced to explain the oc-

currence of pneumonia, causes capable of inducing local inflammation in various parts of the body; but this I am willing to affirm, that, excluding cold, there is no case on record, carefully, completely, and authoritatively reported whose cause can be proved to have been one of the many capable of producing inflammation in other situations. The opinion has long been held that pneumonia is a simple inflammation of the lungs. But I ask, if this is so, is there any cause which is able to bring about an inflammation at any other seat which can produce a true croupous pneumonia? Not one. It does not follow traumatism; it has never resulted from the inhalation of noxious gases or the presence of a foreign body; it has never been due to extension. When inflammation has ensued, following the action of these causes, it has always been catarrhal, a lobular or broncho-pneumonia, not a lobar or croupous pneumonia.

3. The supposed great cause of pneumonia is cold, and so firm was the belief of the older writers in the activity of this agent, that they came to formulate the axiom: "*Frigus unica pneumoniae causa est*," "Cold is the only cause of pneumonia" (Hildebrandt Mareus). But cold as a *supposed* cause of disease is used with such great laxity in the explanation of many and various maladies, that its etiological powers may well be looked upon with distrust and suspicion. In fact, most of us, when at a loss for a cause, almost intuitively turn to cold, the chilling of the surface; and fine-spun theories have been invoked to explain its action. Nowhere, except in the lungs, has cold been supposed able to produce such a peculiar croupous inflammation with such marked constitutional symptoms. Perhaps you will point to laryngeal croup, but even at the present day its etiology is very obscure, while there are many who consider it a mere form or variety of diphtheria, subject to certain definite changes, dependent upon situation, etc. In fact, in

Reynolds' System of Medicine it is classed among the general diseases or zymoses. Besides, the analogy does not hold good in many respects, differences existing, the detailing of which need hardly detain us here.

a. Grisolle was only able in one-fourth of his cases to trace a connection between cold and croupous pneumonia. Chomel and Andral arrived at about the same result. Ziemssen found it active in only one-tenth of his cases in children. In 53 patients observed by Fox, a chilling could be affirmed in only 16. "Close rooms and bad air more predispose than does out-door exposure, unless that be prolonged, or the individual resistance weakened by fatigue or intemperance" (Squire, *loc. cit.*); and this admission by a confessed opponent of the infectious theory. Sturges (*St. George's Hosp. Rep.*, v, 1870) says: "As regards the influence of temperature, it would seem that cold does not necessarily affect the pneumonia rate, but always and markedly the bronchitis rate;" while Lavarán believes that pneumonia depends neither upon the cold season nor upon cold climates.

We all know how prone most people are to ascribe any illness, especially a serious one, to some almost forgotten exposure to cold, in the same way that a traumatism can almost always be discovered in children to account for the Pott's disease or morbus coxarius they may be suffering from. Mrs. Caudle, it will be remembered, contracted her fatal illness by exposure to a cold draught from a key-hole while waiting up one night, many years before, for Caudle who had gone to his club. Of twenty-five cases coming directly under my notice, careful attention having been directed to this point, in only one could a direct relation be traced between a chilling and the lung trouble, and even in this case I am rather inclined to believe that the pneumonia preceded the exposure. An old woman, 72 years of age,

while apparently suffering from great mental depression, for which no assignable cause could be discovered, attempted suicide by drowning, March 8, 1878. She was received into hospital in the evening, immediately after the attempt, wet and cold, and suffering from extensive pulmonary œdema, and delirious. She was energetically treated, the œdema disappeared, the delirium, however, persisted. She passed a fairly quiet night. The next morning early her temperature was 41.1°C (106°F .) and she presented all the symptoms, both rational and physical, of a well-developed acute lobar pneumonia, from which she died the following day. My impression is that the patient, while delirious from an existing pneumonia, made the attempt at suicide, the time intervening between the immersion and the consolidation being seemingly much too short. Juergensen refers to somewhat analogous cases.

The inactivity of wet as an agent in the causation of pneumonia is conclusively shown by the researches of Dr. Octavius Sturges. "Any considerable amount of wet," he says, "has a tendency to heighten the bronchitis rate, but has no such tendency as regards the pneumonia rate; the very lowest numbers of this latter, out of ten years, being found to follow weeks of excessive rain" (*loc. cit.*, p. 141).

b. It has been affirmed that the relation between latitude, season of the year, and the disease in question is not definite, the malady occurring in all climates, and at all times of the year; "pneumonia," says Squire (*loc. cit.*), "is not specially a disease of cold weather or of cold climates." Thus, it is shown to be not more frequent in Iceland than in Jamaica (W. I.) while being far more common in the Bahamas. Hamburg, New York, and Copenhagen exhibit about the same mortality. In some of the expeditions to the North Pole, it has been almost unknown; as also during Napoleon's retreat from Moscow. It is very common

on the high table-lands of Mexico ; while it is far more common in the Southern than in the Northern States. No stronger argument, I think, can be urged against cold as a factor in the production of pneumonia than this fact of the excess of cases in the South over those in the North. This, the following table, based on the eighth and ninth census reports, conclusively shows :

STATES WHOLLY OR IN GREAT PART ABOVE THE 39TH PARALLEL.			STATES WHOLLY OR IN GREAT PART BELOW THE 39TH PARALLEL.		
	PER 1000 DEATHS.	PER 1000 INHAB'S.		PER 1000 DEATHS.	PER 1000 INHAB'S.
1. Maine	51.22	0.62	1. Delaware	56.41	0.68
2. New Hampshire	62.46	0.96	2. Maryland	59.59	0.70
3. Vermont	58.58	0.59	3. Virginia	75.66	0.94
4. Massachusetts	56.29	0.98	West Virginia	71.34	0.80
5. Rhode Island	58.23	0.78	4. North Carolina	102.58	1.26
6. Connecticut	55.96	0.72	5. South Carolina	99.54	1.17
7. New York	51.61	0.87	6. Georgia	113.55	1.39
8. New Jersey	44.84	0.58	7. Florida	123.81	1.47
9. Pennsylvania	60.27	0.65	8. Alabama	127.21	1.68
10. Ohio	80.66	0.88	9. Mississippi	94.15	1.75
11. Indiana	77.94	0.96	10. Louisiana	105.43	1.58
12. Illinois	69.64	0.67	11. Texas	183.42	2.98
13. Michigan	54.55	0.50	12. Arkansas	112.13	1.49
14. Wisconsin	55.30	0.39	13. Kansas	78.49	0.95
15. Minnesota	75.32	0.61	14. Kentucky	84.03	1.04
16. Iowa	87.30	0.93	15. Tennessee	103.96	1.41
17. Nebraska	55.76	0.34	16. Missouri	81.30	1.18
18. Oregon	50.67	0.48	17. Nevada	46.77	0.65
19. Colorado			18. California	60.87	0.98
Average	61.43	0.69	19. Dis't of Columbia		
			Average	93.70	1.27
			Excess over Northern States	32.27	0.58

That there is a definite relation between latitude and pneumonia, I shall now attempt to show. The disease varies greatly in its geographical distribution from maladies ordinarily accepted as due to the direct effects of cold. Let us take, by way of comparison, a prominent member of the class,—simple bronchitis. This disease increases in frequency in an almost direct ratio the further we advance from the tropics ; a circumstance, as we shall see, not true of pneumonia. On this point, Riegel makes the following remarks (Ziemssen's Cyclop. of the Pract. of Med. vol. iv, p. 302):

“The generally accepted opinion that the geographic

extension of diseases of the respiratory organs, bears a direct relation to geographic latitudes, so that there is a decreasing frequency from a maximum at the poles toward a minimum at the equator, does not apply, according to Hirsch, to all diseases of the respiratory organs, but only to catarrh and bronchitis especially. The geographic distribution of the latter affection certainly exhibited *a uniformly decreasing frequency from cold regions toward the tropics.*" Ziemssen and others are also of the same opinion. It is noted that pneumonia is rare in Egypt, though bronchitis is common in the valley of the Nile (Fox).

Conversely to this geographical distribution of bronchitis, pneumonia, all other things being equal, increases uniformly in frequency the nearer we approach the tropics. That is to say, and the fact is a surprising one, and one directly opposed to its presumed dependence on cold, the disease is more commonly met with, is more frequent, in warm than in cold climates, and in hot than in warm climates, showing a gradually increasing ratio from the poles to the equator. Such is undoubtedly true of the United States; and, as far as shown by the statistics I have been able to gather, which, it must be confessed, are not as numerous as could be wished, also of foreign countries. The statistics on which I base the foregoing statements are the following, and are presented without further comment :

COUNTRIES.

	Latitude.	Per 1,000 Inhab.	Per 1,000 Deaths.	Mean An- nual Temp.
Italy	35° 30' - 47° 6'	2.75		60° F.
France	42° 20' - 51° 5'		52.85	55°
“ Cities		1.66	70.8	
Switzerland	45° 48' - 47° 48'	1.54	71.58	57.6°
German Empire	47° 20' - 54°	1.34	60.0	43° to 51°
England	49° 57' - 55° 47'	1.08	48.16	49.5°
Ireland	51° 28' - 55° 23'	0.30	20.3	50°
Scotland	54° 42' - 58° 40'		35.3	47.5°

UNITED STATES.

State.	Mean Annual Temp.	Latitude.	Per 1,000 Inhabitants.	Per 1,000 Deaths.
Minn. . . .	36.2°	43°30'–49°	0.39	55.30
Me. . . .	42.1°	43°–47°30'	0.62	51.22
Vt. . . .	42.1°	42°44'–45°	0.59	55.58
N. H. . . .	40.8°	42°42'–45°18'	0.96	62.46
Wis. . . .	45.9°	42°27'–47°	0.50	54.55
Oregon . . .	52.1°	42°–46°18'	0.34	55.76
Mich. . . .	44.6°	41°42'–48°22'	0.67	69.74
Mass. . . .	48.1°	41°14'–43°53'	0.98	56.29
R. I. . . .	51°	41°18'–42°3'	0.78	58.23
Conn. . . .	49.6°	41°–42°3'	0.72	55.96
Iowa. . . .	47.6°	40°36'–43°30'	0.61	75.32
N. Y. . . .	47.3°	40°29'–45°	0.87	60.28
Neb. . . .	49°	40°–43°	0.93	87.30
Penn. . . .	49.3°	39°55'–42°15'	0.58	44.84
N. J. . . .	51.1°	38°55'–41°21'	0.59	51.61
Dis. of Col.	56°	38°51'	0.98	60.87
Del. . . .	54.3°	38°28'–39°50'	0.68	56.41
Ohio	48.3°	38°21'–41°58'	0.65	60.27
Md. . . .	54.6°	37°53'–39°44'	0.70	59.59
Ind. . . .	53°	37°46'–41°46'	0.88	80.66
* Col. . . .	60°	37°–41°	0.48	50.67
Kan. . . .	52.9°	37°–40°	1.49	112.13
Ill. . . .	48.5°	36°59'–42°30'	0.96	77.94
† Va. . . .	58.6°	36°31'–40°40'	0.94	75.66
Ky. . . .	55.3°	36°30'–39°6'	0.95	78.49
Mo. . . .	53.8°	36°–40°30'	1.41	103.96
‡ Nev. . . .	48°	35°–42°	1.18	81.30
Tenn. . . .	58.5°	35°–36°30'	1.04	84.03
N. C. . . .	60°	33°49'–36°33'	0.80	71.34
Ark. . . .	59.0°	33°–36°30'	2.98	183.42
§ Cal. . . .	56.6°	32°28'–42°	0.65	46.77
S. C. . . .	64°	32°4'–35°13'	1.26	102.58
Miss. . . .	66.4°	31°52'–35°	1.68	127.21
Ga. . . .	55.9°	30°20'–35°	1.17	99.54
Ala. . . .	62°	30°15'–35°	1.47	123.81
La. . . .	62°	28°56'–36°30'	1.75	94.15
Tex. . . .	68°	25°51'–36°30'	1.58	104.43
Fla. . . .	69.6°	24°30'–31°	1.39	113.55

This fact that pneumonia decreases in frequency the further we advance from the tropics, has, as far as I know, never before been pointed out, and on it I would lay a great deal of stress as an argument against the dependence of the disease upon the action of cold. It must be here remembered that large cities show a much larger proportional

* Population by far most dense above the 39th parallel of latitude.

† Includes Virginia and West Virginia.

‡ Population almost entirely above the 39th parallel of latitude.

§ Population almost entirely above the 37th parallel of latitude.

|| Population almost entirely above the 29th parallel of latitude.

CITIES.

	Latitude.	Per 1,000 Inhab.	Per 1,000 Deaths.	Mean An- nual Temp.
1.—Of the United States.				
New Orleans . .	29°56'	1.48	47.1	67.5°
Nashville . . .	36°11'	1.8	57.2	58.47°
San Francisco . .	37°48'	1.06	56.9	56.48°
Philadelphia . .	39°6'	1.26	51.6	52.5°
Cincinnati . . .	39°6'	1.21	62.8	51.37°
Baltimore . . .	39°18'	1.17	23.5	54.64°
Dayton	39°44'		78.0	50.56°
New York . . .	40°4'	1.82	66.8	51.4°
Brooklyn . . .		1.76	69.3	
Pittsburgh . . .	40°32'	1.45	63.9	52.6°
Chicago	41°52'	1.13	46.3	46°26°
Boston	42°2'	1.55	67.2	48.5°
Detroit	42°19'	0.90	49.7	45.5°
Saginaw	42°20'	0.73	62.2	
2.—Of France.				
Bordeaux . . .	44°50'		58.7	
Lyons	45°54'		41.6	
Paris	48°50'	2.3	82.0	51.54°
Lille			26.0	
3.—Of the German Empire.				
Strasburg . . .	48°35'		94.3	49.64°
Stuttgart . . .	48°46'		116.0	49.64°
Wurzburg . . .	49°48'	1.50		50.18°
Frankfort . . .	50°10'		105.2	49.28°
Dresden	51°3'	0.4		
Berlin	52°30'	1.25		48.2°
4.—Of Belgium				
Brussels . . .	50°51'		50.6	50.36°
5.—Of the British Isles.				
London	51°31'	1.28	55.15	48.38°
Dublin	53°20'	0.8		49.1°
Leith			41.2	
Edinburgh . . .	55°57'	1.43	35.4	46.58°
6.—Of Algeria.				
Algiers	36°47'	3.80	108.0	67.5°

death-rate from pneumonia than small cities, which would account for the apparent breaks in the table of cities, such, for instance, as in Paris, New York, and Brooklyn.

Of the very cold climates, but few statistics are at hand, but, as far as they go, they seem to indicate that the disease is rare. Such, at least, is true of Sweden, the Farøe Isles, and Iceland. Niemeyer remarks (*Text-Book of Pract. Med.*, vol. i, 1874, p. 163): "The statistical statements

as to the greater frequency of pneumonia in northerly and elevated localities, have, of late, been regarded as untrustworthy." In many of the expeditions to the North Pole, pneumonia was almost unknown.

Professors Hirsch and Ziemssen have shown that pneumonia occurs much less frequently in sparsely inhabited though cold districts than in more populous and warmer climates; in fact, as long ago as 1858 the latter formulated the law, that "the amount of the mean fluctuation in the mortality from pneumonia is in inverse ratio to the density of the population." Accepting the foregoing as true, and statistics certainly bear it out, that such is not true of the ordinary inflammatory diseases might easily be proven, but it will simply suffice here to show that the simple phlegmasiæ of the lungs obey no such rules. The statistics are those obtained from the health reports of New York City, and indicate deaths from the various diseases mentioned, proportioned to every 1,000 population :

YEAR.	BRONCHITIS.	Pleurisy and other diseases of the pleural cavity.	Diseases of the respiratory organs in local class.	Diseases of the respiratory organs in local class, less pneumonia.	Population per sq. mile.
1865 . .	0.59	—	—	—	39,499
1866 . .	0.62	0.11	—	—	40,123
1867 . .	0.77	0.10	3.09	1.50	40,757
1868 . .	0.87	0.11	3.33	1.51	41,401
1869 . .	0.94	0.11	3.76	1.50	42,055
1870 . .	0.90	0.12	3.36	1.42	42,715
1871 . .	1.01	0.10	3.40	1.48	43,275
1872 . .	1.07	0.11	3.77	1.55	43,841
1873 . .	1.09	0.08	3.91	1.54	44,416
1874 . .	1.07	0.09	3.96	1.55	44,978

Showing a steady increase in the death-rate of bronchitis directly proportioned to the increase in population—about .098 per 1,000 increase in population per square mile. The

death-rate of pleurisy, and all diseases of the respiratory organs, excluding pneumonia, belonging to the "local class," remains stationary. We see the same steady increase in the loss from bronchitis in other localities. In England we have the remarkable fact presented of a death-rate from bronchitis more than doubling that from pneumonia. This excess is not only true for England, but also, though to a less degree, for Scotland and Ireland. However, it has not always been so. Thus, in 1838, the death-rate from bronchitis, in England, was 0.14 per 1,000 inhabitants, or 6 per 1000 deaths from all causes, while that of pneumonia was 1.2 per 1000 inhabitants, or 52.6 per 1000 deaths; that is, the latter was $8\frac{1}{2}$ times more fatal than the former. From this time on we find the mortality from bronchitis steadily increasing, that from pneumonia remaining, save for its annual fluctuations, about stationary. In 1855, we find, for the first time, the deaths from bronchitis more numerous than those from pneumonia, followed in the next year by a slight preponderance of the latter. Since then the pneumonia death-rate has at no time exceeded that of bronchitis; and we now find the proportions, (returns for 1876,) to be: pneumonia, 0.74 per 1,000 inhabitants, or 48 per 1,000 deaths; bronchitis, 1.63 per 1,000 inhabitants, or 105.9 per 1,000 deaths; that is about $2\frac{1}{4}$ deaths from the latter to one of the former. Turning to London, we find the relations, in 1838, as follows: bronchitis, 0.23 per 1,000 inhabitants, or 8.6 per 1,000 deaths; pneumonia, 1.42 per 1,000 inhabitants, or 71.05 per 1,000 deaths. In 1850, the bronchitis rate for the first time exceeded that of pneumonia, that is five years before the same became true of England, as a whole. The proportions now stand, (returns for 1876,) bronchitis 2.71 per 1,000 inhabitants, or 121.6 per 1,000 deaths; pneumonia, 1.21 per 1,000 inhabitants, or 54 per 1,000 deaths; that is, a ratio of $2\frac{1}{4}$ to 1 in favor of bronchitis. We here

notice the fact, to which attention has already been directed, that given a certain region, whether city, state, or country, all other things being equal, with a steady increase in population there is a corresponding steady increase in the mortality from bronchitis, over and above that related to the proportioned actual increased mortality from all causes. This fact, as far as statistics show, is also true of most or all our great cities. Pneumonia obeys no such law. Though subject to marked annual fluctuations its death-rate remains about the same.

We now turn to the bearing of the seasons on this question. Taken the world over, pneumonia may be considered a disease of the winter and spring months. Variations, however, will be found to exist in different localities and in different years. Thus, for Stockholm (Huss) we have, 2,616 cases during sixteen years, the spring months giving 49 per cent., the winter 30 per cent., and the summer 21 per cent.; the months April and May showing the greatest prevalence of the disease; August and September, the least. In Edinburgh, Dublin, and Stuttgart it is most common during the winter; in Copenhagen, Berlin, Zurich and, Turin, during the spring. It is least prevalent in the summer in Edinburgh, Dublin, Zurich, Wurtzburg, Stuttgart, and Turin; while in Copenhagen, Berlin, and Vienna it reaches its minimum during the autumn. In many parts of Europe it is spoken of as a "May epidemic" (Squire). In our own city, most of the fatal cases are recorded during the winter and autumn months, least during the summer.

In our own country the disease is common during the vernal months, and, excepting the winter, is comparatively infrequent at other seasons. It is most frequently met with during the month of March, least during July. However, differences, both as regards season of the year and month, are found to prevail over various parts of the country.

	United States.	District I.*	District II.	District III.	District IV.	District V.	District VI.	District VII.	District VIII.	District IX.
January . .	12.7	10.3	12.7	10.4	11.7	12.0	12.0	15.4	15.9	11.3
February . .	13.9	12.1	14.3	14.6	15.6	13.6	13.8	15.0	13.7	10.8
March . . .	16.0	15.7	17.3	16.3	18.4	16.3	15.9	14.3	14.4	12.7
April . . .	12.6	13.0	11.4	13.1	12.8	13.2	13.5	11.2	12.3	8.4
May	9.4	10.8	9.6	9.9	8.4	11.3	8.2	9.9	8.7	12.1
June	4.7	6.4	4.9	5.8	4.1	4.8	4.2	4.1	3.5	3.7
July	3.3	3.8	3.7	3.3	3.0	3.9	3.1	3.2	2.9	4.7
August . . .	3.5	3.6	3.8	3.4	3.4	3.8	3.6	2.9	3.4	5.6
September . .	4.0	4.2	4.4	3.9	4.0	3.3	4.2	3.8	4.0	6.5
October . . .	5.0	4.9	4.6	4.6	4.7	4.2	5.5	5.2	5.1	7.5
November . .	6.5	7.2	6.0	6.7	6.2	6.0	6.8	6.1	6.3	6.9
December . .	8.4	8.0	7.3	8.0	7.7	7.6	8.3	8.9	9.8	9.8

DIST. I.—Me., N. H., Vt., Mass.,
Conn., R. I., and N. Y.
“ II.—Mich., Wis., Minn., and
Neb.
“ III.—N. J. and Penn.
“ IV.—O., Ill., Ind., Iowa, and
Kan.
“ V.—Del., Md., Dis. of Col.,
Va., and N. C.

DIST. VI.—Ky., Tenn., Mo.
“ VII.—S. C., Ga., Fla., and
Ala.
“ VIII.—Miss., La., Ark., and
Tex.
“ IX.—Cal., Oregon, Nev., and
Col.

Eighth Census Report, volume on Vital Statistics.

	1st quarter.	2d quarter.	3d quarter.	4th quarter.
United States	42.6	26.7	10.8	19.9
District I	38.1	30.2	11.6	20.1
“ II	44.3	25.9	11.9	17.9
“ III	41.3	28.8	10.6	19.3
“ IV	45.7	25.3	10.4	18.6
“ V	41.9	29.3	11.0	17.8
“ VI	42.6	25.9	10.9	20.6
“ VII	44.7	25.2	9.9	20.2
“ VIII	44.0	24.5	10.3	21.2
“ IX	34.8	24.2	16.8	24.2
Northern States	42.3	27.6	11.1	19.0
Southern “	43.3	26.2	10.5	20.0

* “ The nine districts, with the exception of the ninth, or Pacific region, severally represent the different characters of the country.”

Juergensen claims that insular and continental climates differ as regards the time of greatest prevalence, being most common in the winter, in the former ; in the spring, in the latter. This certainly seems true of Europe, but when applied to our own country, the conclusion is found untenable. Now, the fact that a disease is most frequent during a certain part of the year is, of course, presumptive evidence, on the face of it, that a definite relation exists between the two. But the same relation may be predicated of many of the acute infectious diseases. Besides, these diseases also appear at different periods in different countries. Thus, typhoid is most prevalent during August, September, October, and November (70 epidemics, against 46 during the other eight months. Hirsch). In England it is most common in autumn, during September and October ; while among us it is known, more especially in New England, as autumnal fever. It is also remarked that with a low degree of temperature the maximum of the malady is high. Cholera is particularly frequent during the summer, chiefly July and August, while there is a remarkable exemption from it in winter. Measles is met with most frequently during the winter ; scarlatina and variola, in the autumn. Now, cerebro-spinal meningitis, a disease which at one time was classified among the local inflammatory disorders, generally occurs during the winter and spring months, more especially with great variations of temperature and the moisture of the air (Ziemssen) ; yet no one now believes the disease to be one resulting from the action of cold. And so it is with epidemic influenza, a catarrhal process. There was a time when this disease was presumed to be dependent upon a low temperature, or sudden variations of temperature, but at present no one entertains such a belief.

A single glance at the tables given will show that, al-

though pneumonia reaches its maximum of mortality during the first and second quarters of the year, this can hardly be due to the fact that these embrace the coldest months, since it prevailed with about equal frequency in the various districts, the warm as well as the cold, during the various periods of the year; in fact was slightly more common in the Southern states during the winter quarter than in the Northern.

The deduction from the foregoing, that season, whether of low or high temperature, bears no constant or direct relation to the prevalence of croupous pneumonia, is, I think, a fair one. But, claim the advocates of cold, it is not the continuously low ranges of temperature which are associated with the disease, but rather it follows rapid and marked variations or fluctuations, a sudden fall from a comparatively high to a comparatively low. Let us see. Even admitting that variability of temperature is apparently related to pneumonia, but so is it, as I have already shown, to a number of the so-called essential fevers. Besides, the relation is far from uniform. In Senegal, a country possessing a variable climate, the disease is rare, as also in Sweden and the Farøe Isles; while in the Bermudas, having a remarkably equable climate, it is far from uncommon. As long ago as 1847, Dr. Farry (*Am. Journ. Med. Sci.*, 1847) showed that pneumonia is less common in the Northern and Southern states than in the more central portions of our country, where the temperature is more uniform. This fact still holds good, the largest mortality from the disease being found in Arkansas, a central State. Oregon and California enjoy, to a great extent, immunity from the disease. Besides, as has already been shown, the malady is very common in the Southern states, where actual cold seasons and abrupt, sudden changes are almost unknown.

Again, comparing pneumonia with bronchitis, we will find

a well-marked difference in their respective relations to seasons. Riegel (*loc. cit.*, p. 305) states "it is further worthy of remark, in regard to frequency in individual seasons, that there is a want of coincidence between croupous pneumonia and catarrh of the respiratory organs, as is evident from different series of statistics, especially those of the general hospital at Vienna." We have but to present the following statistics to show the correctness of this assertion :

	1st quarter.	2d quarter.	3d quarter.	4th quarter.
United States—Pneumonia .	42.6	26.7	10.8	19.9
—Bronchitis .	34.1	27.1	16.4	22.4
N. York City—Pneumonia .	34.0	26.0	14.3	25.7
—Bronchitis .	32.6	24.5	15.4	27.5
New Orleans—Pneumonia .	31.3	25.1	20.6	23.0
—Bronchitis .	35.9	23.1	17.9	23.1
London —Pneumonia .	30.9	23.7	14.6	30.8
—Bronchitis .	38.3	23.6	11.2	26.9
Dublin —Pneumonia .	31.7	26.6	16.2	25.5
—Bronchitis .	44.5	21.9	9.7	23.9
Paris —Pneumonia .	30.8	28.4	18.3	22.5
—Bronchitis .	34.7	25.8	13.2	26.3
Lyons —Pneumonia .	40.2	22.8	18.5	18.5
—Bronchitis .	50.4	20.2	10.1	19.3

Pneumonia is less common among those who lead an out-of-door life than those whose occupations keep them within doors. No positive and reliable data are at hand which enable us to decide the relations of the various conditions of life, occupations, professions, etc., to pneumonia ; but it has been broadly stated that those whose labors are chiefly or entirely out-of-doors do not suffer more than those whose labors are under cover ; whereas, the fact stands, that what few statistics are at hand tend to show that the morbidity and mortality from the disease are less among the former than the latter. That such is the case is proven by the statistics of Oesterlin ("Handbuch der Medicinischen Statistik," 2te Ausgabe, Tübingen, 1874), Hirt (Die Krank-

heiten der Arbeiter), and Hannover (*Monatsblatt der Deutschen Klinik f. Medic. Statist., etc.*, No. 6, 1861).

It is less frequent among country than city dwellers, while occupants of prisons and cloisters present a high rate of mortality. Soldiers in the field and sailors at sea suffer less than when in garrison or on land. During Napoleon's retreat from Moscow but few of the soldiers died of pneumonia, though thousands perished from cold. I have dwelt thus at great length upon the various relations of cold to pneumonia, recognizing its manifest importance in the solution of the question of the nature of acute croupous pneumonia. The conclusion that the power of cold as a causative agent of the disease stands, as the Scotch verdict has it, "not proven," is, it must be conceded, warranted by the foregoing facts.

4. I now turn to the symptoms of the disease. If pneumonia be a purely local phlegmasia, a parallelism should exist, as really does in cases of this group of diseases, between the local lesion and the constitutional symptoms. Mild symptoms should accompany circumscribed lesions; aggravated symptoms, extensive involvement of tissue; modified, of course, by the relative importance in the economy of the organ diseased. For instance, we have the following case: A patient receives a small contused wound of the leg, following which we have local symptoms; inflammation with perhaps some circumscribed sloughing, but no, or at least only almost imperceptible, constitutional signs. Again, a patient is suffering from a compound fracture of the same part. The local disturbance is great, the constitutional symptoms marked, but still directly proportioned to the severity of the injury and reaction therefrom. As soon as the constitutional symptoms become disproportioned to the degree of local trouble; if, for example, we note repeated chills, followed by high fever with all it entails; if we

have copious sweats, sweet breath, etc., do we look to the injury itself to account for these constitutional manifestations? No! We search deeper, and we find what? This: our patient is suffering from self-poisoning; pus or its elements have been absorbed into his blood, thus affecting his entire organism by an auto-infection; he is now the victim of a constitutional malady, pyæmia. We would not expect a localized pleurisy, or a slight bronchitis to present the same intensity of symptom as when more widely diffused. I might multiply examples of purely local inflammatory complaints, but should always find the same rule holding good,—the severity of the symptoms directly related to the degree and extent of the lesion, subject to the usual modifying influence of the importance of the organ involved. Perhaps acute tonsilitis may be pointed out as an exception to this rule. On this question I quote from Wilson Fox (*loc. cit.* p. 678). “The disorder which, on a lesser scale, presents the greatest analogy with acute pneumonia, is perhaps acute tonsilitis, where we have the same short initial stage, a similar intensity of rigor and prostration, a similar sudden invasion of pyrexia, and a similar rapid decline of this before the local inflammation has shown any signs of abatement. In tonsillitis also we have frequently an equal difficulty with pneumonia in verifying a distinct cause, and a certain amount of evidence at least exists in the case of the so-called ‘hospital sore throat’ that it may also be produced by other poisons than those originating within the system from the impeded exercise of the functions of the skin.” We have been having during the last few years in this city a peculiar non-diphtheritic tonsillar angina, possibly contagious in its nature, which is accompanied by well-marked and aggravated constitutional symptoms. Thus, I know of a case in which, at first, no positive diagnosis could be reached, the supposition being that the patient was

afflicted with so violent a disease as cerebro-spinal meningitis. I have but little doubt that in these cases we are dealing with a veritable essential fever, an acute infectious disease, and not a purely local trouble.

In the vast majority of cases no relation can be traced between the extent of lung tissue involved in pneumonia and the general symptoms of the disease. Now it is a very noticeable fact in acute infectious diseases that the constitutional manifestations are, as a rule, in no way directly related to the local lesion, as we see in typhoid fever, scarlatina, measles, diphtheria, etc. I have myself seen three walking cases of pneumonia, consolidation in all being well marked, and of these, one at no time during his illness was confined to his bed.

Premonitory symptoms, preceding the apparent initial sign of the disease, are not uncommon in pneumonia, while absent in true phlegmasiæ. Grisolle found prodromata in 50 out of 205 cases, as general malaise, heaviness of the limbs, anorexia, etc., these lasting during a variable period of from a few hours to several weeks. Wilson Fox says they may exist for one or two days or even longer, appearing as malaise, chilliness, loss of appetite, headache, pains and achings in the back and limbs, and an earthy or icteric skin. Traube and Grisolle are of the opinion that pyrexia of a marked kind may sometimes precede the local signs. Upon this point, however, opinions vary greatly. Huss believes in a slight precedent feverishness. According to Fox the only case bearing on this subject is one by Monthus (*Essai sur la pneumonie double*).

A patient in hospital for an abscess of the foot one night got a chill from a draught of cold air; the next morning she felt ill, and the temperature was 38° C. (100.4° F.); a quarter of an hour later she had a chill, temperature at the beginning, 38.3° C. (100.9° F.), during the rigor and for an

hour after 41° C. (105.8° F.), falling later, and remaining at 39.8° C. (103.6° F.). The following day the local signs of consolidation showed themselves, her temperature then standing at 40° (104° F.).

Among 24 cases coming under my charge in Bellevue Hospital during the winter of 1877-78, many gave histories of prodromal signs, but I select the two following: A female, healthy, and for a long time helper about one of the wards, had on Saturday night, January 12th, a severe chill, lasting between one and two hours, and followed by a feeling of great heat. During Sunday she had much nausea and vomiting. She felt chilly for two or three hours in the morning, alternating with flashes of heat, and also noticed that her skin resembled "chicken's flesh." Her temperature was taken in the evening and found to be 39.5° C. (103° F.) in the vagina. The following two days she continued about the same, being, however, able to work about the ward. Tuesday night she had another chill, followed by a cutting pain in the side, and cough; and now for the first time were physical signs of lung consolidation detected, that is, about three days after the appearance of the first symptom. Her temperature was not again taken until Wednesday evening, when she first took to her bed. It was then 38.9° C. (102° F.), vaginal. The diagnosis was verified by *post-mortem*.

A second case was the laundress of the hospital. Here, unfortunately, the temperature record is lost. She was seen several times for two days before the first signs of lung consolidation were discovered, and her temperature, if my memory serves me right, was 39.5° C. (103° F.) on one occasion, and 40.3° C. (104.5° F.) on another. However, I am certain that there was a decided elevation.

The disease in the large majority of cases opens with a distinct, well-marked, often prolonged chill, such as no sim-

ple local inflammation presents. Huss observed it in 80 per cent. of his cases; Lebert, in 92.5 per cent.; Grisolle, in 80 per cent.; Fisser, in 76.6 per cent.; Louis, in 77.2 per cent. Fox states that among 53 patients its entire absence is recorded in 9 only; while in 34,79. per cent. of the positive cases, it was distinctly present. From these figures we obtain an average of 80.9 per cent. for all cases marked by a chill.

The type of fever characteristic of pneumonia totally differs from that of a purely local disease. Dr. James Andrew claims (*Med. Times & Gaz.*, vol. xxxv, 1877, p. 130) "that lung consolidated without a certain type of fever, has no more right to be called pneumonia than fever without consolidation. In fact, of the two, the latter would be nearer the truth." Rapidly following the initial symptom, there is a sudden and considerable rise in temperature, frequently in a few hours reaching 40° C. to 40.6° C. (104° F. to 105° F.). The point reached is maintained throughout the course of the disease, marked only by morning remissions and evening exacerbations, averaging from 1° C. to 2° C., followed by a sudden and abrupt fall to the normal on defervescence.

The height of the fever does not depend upon the extent of lung tissue involved, and, says Dr. Parkes, (*Med. Times & Gaz.*, vol. i, 1860, p. 186), complete consolidation seems to be posterior in point of time to the height of the pyrexia. According to Bernheim (*Revue Méd. de l'Est*, No. 1, 1877, p. 11), "the fever precedes and governs the local lesion; the one falls, the other resolves." "What," asks Prof. Wm. H. Draper (*Bulletin of the N. Y. Acad. of Med.*, vol. ii, 1866, p. 519), "is the exact relation of the pyrexia to the pulmonary lesion at the commencement of the disease? Is the lesion the initial step in the malady and the primary cause of the pyrexial symptoms? or is the

pyrexia the essential element of the disease, the lesion being a secondary phenomenon? It is impossible to answer the question definitely; but we think the experience of careful observers will agree that in many cases, which are seen very early, the pyrexia precedes by some hours, occasionally by a day, any indication of pulmonary lesion appreciable to physical exploration."

Small pneumonic consolidations are often accompanied by high ranges of temperature, extensive consolidations by low ranges. I have seen a case where the physical signs were confined always, on repeated examinations, to a region high up in the right axilla, over a space about as large as an old copper penny; yet in this patient the temperature ranged between 39.5° C. (103° F.) and 40.4° C. (104.75° F.). I have also seen cases with extensive involvement of lung in which the temperature ranged in the neighborhood of 38.9 C. (102° F.). Apex pneumonias are marked by a high fever; thus, as in a case I have met with, where the thermometer registered 41.7° C. (107° F.) in the axilla. This patient recovered. In fatal cases there is not infrequently a considerable rise before death, as in the case of a child coming under my notice, reaching 42.8° C. (109° F.), rectum. Such a type of fever, as above described, differs completely from that of a simple local inflammatory process.

Herpes, an eruption seldom if ever seen during local inflammatory troubles, is very common, but occurs with varying frequency in different years. Its most frequent seat is the lips and angles of the mouth, appearing on from the 2d to the 5th days of the disease. It is more commonly met with in this malady than in any other febrile disease, not excepting intermittent fever. During my term of service in Bellevue Hospital, I well remember that the first winter almost every case of pneumonia admitted, presented herpes labialis; so much so that I almost came to

regard it as a necessary phenomenon of the disease. But during my second winter not a single case under my charge presented this symptom.

Head symptoms are more prevalent in pneumonia than in ordinary inflammatory disorders. Pneumonia, like many of the infectious diseases, as typhoid, typhus, etc., presents a peculiar and characteristic facies. A flushed face, especially over the malar prominences, with an earthy tint of the skin around the eyes and lips, is so common that I have more than once seen a diagnosis made on the appearance of the face alone. Sweating is another symptom not usually found in local inflammatory complaints, but frequently existing during the course of the disease in question. In this it resembles the infectious maladies, intermittent fever, and pyæmia. It involves the entire body, is quite frequently accompanied by sudamina, and seems to have a critical significance. In some instances I have seen it as profuse as in acute articular rheumatism. Albuminuria is quite common and seems to indicate a mild variety of nephritis.

No simple inflammatory disease is marked by defervescence or crisis. Pneumonia in the vast majority of cases terminates in this way. For example, among 27 cases (Fox) 20 terminated thus. It may take place as early as the second day, or may be delayed until the eighteenth (Lebert). The crisis does not seem to be dependent upon the condition of the lung, for it may occur while the inflammation is still at its height (Fox).

5. The morbid anatomy of the disease also supplies us with arguments tending to disprove the local nature of the malady. The exudation of croupous pneumonia is peculiar in its extent and amount, occurring in no other disease of the lungs, nor in any simple inflammatory trouble with which we are acquainted. Catarrhal pneumonias are

lobular and scattered; croupous, lobar and compact, often involving a whole lung; in catarrhal pneumonias the exudation is comparatively small in amount; in croupous pneumonia, large, reaching from $\frac{1}{2}$ to 1 kilo. when single-lobed, and 2 kilos. with involvement of an entire lung. This exudation entirely differs from that of ordinary inflammation, and, as has been seen, cannot be obtained artificially. The fact that the exudation undergoes liquefaction and absorption, being expectorated only to a slight degree, and not infrequently not at all, the air-vesicles, as far as we know, being left in a condition as perfect as preceding the disease, distinguishes it also from the inflammatory disorders of the lungs, where the contrary is found to prevail.

The relation of the lung consolidation to the constitutional phenomena we have already noted and need only refer to here. Its tendency to attack certain lobes, the lower, and most frequently the right, is presumptive evidence that the cause lies deeper than a simple local irritation. Stokes (*A Treatise on the Diagnosis and Treatment of Diseases of the Chest*, 2d ed., Philadelphia, 1844) relates that he has observed an epidemic tendency to pneumonia of the upper lobes, and further states that in the summer of 1833 a great number of cases of this description occurred in the Meath Hospital.

The associated morbid changes may, with certain reservations, be urged as against the supposed local nature of the disease; as also the complications. Catarrh of the gastro-intestinal mucous membrane, with enlargement of the solitary and Peyerian glands, is common; but as this is also true of most diseases attended by prolonged high fever, whether infectious or not, this argument is advanced with reserve. The liver is usually congested and enlarged, the spleen congested, softened, and enlarged by about one-

half,—appearances generally noticed in most acute febrile troubles, more especially those characterized by a continuous prolonged high temperature. Catarrh of the pelvis of the kidney is an almost constant lesion, and acute parenchymatous nephritis, usually mild in character, is far from uncommon. The brain is generally simply congested; sometimes meningitis is present. The muscles are dry and brownish-red, at times presenting the granulo-waxy change so common in typhoid fever. The same is true of the muscular tissue of the heart.

As a matter of curiosity, I note the fact that pneumonia, like some of the infectious diseases has several times been observed during intra-uterine life (F. Weber, "Path. Anat. des Neugeb. und Säuglinge," ii, 41, *et seq.*).

6. Even our very treatment is contrary to our preconceived notions of the management of a strictly local disease. Time was when theory and practice agreed, the antiphlogistic being the method employed to combat the disease. But at present, excepting perhaps a few of our older practitioners, it is totally discarded. In no essential does our present mode of treatment differ from that of the infectious diseases, for instance, typhoid fever: cold bathing, quinine, salicylic acid perhaps, stimulants when necessary, and careful regulation of the diet and surroundings. We treat the disease through its symptoms, constitutionally, not locally as we would a local inflammation. Perhaps it will be urged, but do we not employ some local means? To this I would answer, we do, but only against local symptoms or local complications, in the same way that we resort to similar measures in typhoid fever, scarlatina, diphtheria, and the like. We are then not treating the disease itself, but rather such local manifestations as may demand attention.

(To be continued.)

THE FLUORIDES IN MEDICINE.

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IT has often occurred to me as strange that with preparations in such daily use as those of chlorine, bromine, and iodine, the other member of the group, fluorine, should be so completely ignored. It is true that its known irritating character, and its properties of corroding glass, seem to point it out as a substance whose use is to be shunned. But others of the group are also most irritating and difficult of control. Yet their salts are more actively employed and more esteemed than perhaps any articles in the medical armamentarium. This reasoning led me to having some of the salts of fluorine made; and an accomplished pharmacist, Mr. McKelway, with whom I communicated about a year ago, produced, after several trials, a number of salts of which I here append a short description.

These salts were all made from the purest attainable hydrofluoric acid, and in silver vessels. Some samples of acid contained traces of lead, and this rendered great care in the selection necessary. To be certain of purity, the commercial acid should be re-distilled or made in silver or platinum vessels. The acid can be readily preserved in gutta-percha bottles. The salts Mr. McKelway prepared were the potassium fluoride, sodium fluoride, ferrous fluoride, and ferric fluoride.

The potassium fluoride was obtained by simply saturating the hydrated fluoric acid with potassium bicarbonate, and evaporating to dryness. A deliquescent salt results, soluble in three parts of water, and having a not unpleasant saline taste. In concentrated solution it dims glass. It is insoluble in alcohol, and is precipitated by it from aqueous solutions. If an ordinary solution be kept for some time a little flocculent deposit takes place in the bottle.

The potassium salt is by far the best to use, being the most soluble.

The sodium fluoride is prepared in precisely the same manner, substituting, of course, sodium bicarbonate for potassium bicarbonate. The salt is very slowly soluble in about twenty-five parts of water and is infinitesimally dissolved by alcohol. The taste is purely saline, very much like sodium bromide, and even less disagreeable. Because of the weakness of the solution, it dims glass but slightly on long standing.

Ferrous fluoride was made by dissolving iron in the warmed acid to saturation. By evaporating at a gentle heat, a yellowish-white salt is obtained, very slowly soluble in about eight parts of water. It is permanent in the air, and has a sharp styptic taste.

Ferric fluoride results from dissolving freshly precipitated hydrated ferric oxide in the acid to saturation, and evaporating. It is a light brownish-yellow salt, slowly soluble in about twenty parts of water, and having a sweet mawkish taste.

From these salts, especially from the potassium fluoride and ferrous fluoride, various preparations were made: elixirs for the most part with rectified spirit and with the oil of the bitter orange and of cinnamon; or syrups, with syrup of orange flowers, and, in the case of the iron salt, with the addition of a few grains of citric acid to a four-

ounce mixture. Glycerine may also be used in these liquid preparations, but those made with it were not as agreeable.

Having described the chemical and pharmaceutical properties, I shall now speak of the fluorides as medicines. With the most valuable aid of Dr. Jimenez, one of the resident physican at the Pennsylvania Hospital, I observed their effects in a number of cases. And let me say at once that observations with the sodium salt were speedily abandoned, as even in doses of three grains it produced, after a few doses, nausea, vomiting, thirst, and some burning pain at the epigastrium. Both the ferric fluoride and the ferrous fluoride were at different times employed; the latter proved the more pleasant, and was more thoroughly tested.

With reference to the potassium fluoride, these results were obtained: It was first given to a case of subacute rheumatism, with considerable pain, in doses of five grains every third hour. It produced a most decided effect on the pain, without causing drowsiness or showing any influence on temperature. The urine was increased, and the specific gravity lowered; but this may have been owing to the greater amount of water taken in consequence of the thirst. At the end of the second day there was nausea, with loss of appetite, and the medicine was discontinued. The pains returned; and at the patient's own request, a few days subsequently, the fluoride was resumed, in five-grain doses every third hour, and continued for three days, with the same good influence on the pains, but with, finally, the nausea and epigastric distress, which compelled its discontinuance. No hypnotic properties were observed from its employ.

In a second case of rheumatism, more acute than the first, five grains were given every third hour. The pains were greatly lessened, but the same gastric uneasiness was caused as in the first case, and also occasional burning pain and,

once, vomiting. On the third day the bowels became loose, and the medicine was abandoned. But the patient missed the relief from pain, and asked that the salt be resumed. It was done; yet, as after a few days some gastric distress was noticed and loss of appetite, it was finally stopped. During its administration the flow of urine was greatly increased, the patient drinking, however, considerable water. Before the salt was given, three pints of urine were passed daily; on the second day, six pints; on the third day, five pints; the specific gravity was lowered. In this patient accurate observations on pulse, respiration, and temperature were made. At 3 P.M. the pulse was 100; the respirations were 24; the temperature was 101° F. Ten grains of the fluoride potassium in solution were then given.

An hour afterward: pulse, 100; resp., 26; temp., 101°.

2 hours	"	"	102	"	24	"	100°.
3	"	"	100	"	24	"	99.5°.
4	"	"	90	"	24	"	100°.

In other cases similar observations were repeated: the temperature was little, if at all, influenced, even where the medicine was continued, in five grain doses, three hours apart, and occasioned nausea; half a degree, or in one instance one degree, representing the maximum depression; the pulse was slightly lowered, but its volume not materially decreased.

In an inveterate case of sciatica the same reduction of pain was perceived without sleep being induced. But here the medicine nauseated, even in grain doses, and could not be taken for more than a few days.

In larger doses than five grains the medicine is apt to be an emetic, and from ten to twenty grains will, in most persons, quickly cause vomiting, with a moist skin, a slightly reduced pulse, but without subsequent depression. Indeed, I do not know a prompter emetic than fluoride of potas-

sium is in these doses. The short stage of nausea, or its almost entire absence, was several times noticed.

A saturated solution of the potassium fluoride applied to the skin under waxed paper, did not seem to have any irritating qualities.

Of the iron preparations, the sesquisalt or the *ferric fluoride* produces in two-grain doses, after four or five days, loss of appetite and nausea, while the stools are blackened as from preparations of iron, and the other secretions unchanged. The protosalt, the *ferrous fluoride*, administered in solution or in elixir or syrup, is much better borne, and while all the usual evidences of giving iron are manifest, it does not constipate. But it did not prove a powerful agent in influencing nutrition, as the iodide of iron often does; and, while agreeable to take, it cannot be continued for a very long time, as loss of appetite, gastric uneasiness, sometimes nausea, interfere with its use.

Tried in several instances of both pneumonic and tubercular phthisis, in which at first it seemed to be answering, it had, finally, for the reasons assigned, to be discontinued.

Reviewing, now, these observations, I find that the fluorides, especially the fluoride of potassium, are, in large doses, prompt emetics, without depressing. They seem to relieve pain—again I refer particularly to the fluoride of potassium—without producing the markedly quieting and sedative effects on the nervous system that the bromides do. The difficulty with them is to give them long enough to test their influence on nutrition and absorption; to test, for instance, whether they promote healthy tissue-formation, and would be useful in diseases like scrofula and consumption, or lead to absorption of glands as iodine does of goitres.* They are not well enough tolerated by the stom-

* As these pages are passing through the press, I find in an article by Woakes on goitre, concluded April 2, 1881, *London Lancet*, some observations with a half-per-cent. solution of pure fluoric acid, in fifteen- to thirty-minim doses three

ach, I fear,—at least I have not yet succeeded with any of the preparations tried to bring about this tolerance,—to be really useful additions to practical medicine, especially not in the treatment of long-continued or of chronic diseases. Their most singular effect—this is manifest from the iron salts—is the anorexia that in small doses they produce, with but slight nausea, and depression. It may be, therefore, that in instances of bulimia they may come into play, or that they may be employed to counteract the craving for drink. They increase the desire for water, but would destroy with the desire for food the desire for alcoholic stimulants. But these ara, on the whole, limited uses, and while there are properties of value in the fluorides, my trial of them makes me believe that their applicability in medicine and their therapeutic employ will not be great.

times daily. The medicine was borne sufficiently long to produce instances as striking, or even more striking, of the absorption of goitres as when iodine is administered. But looking closely at the cases reported, I observe indications of the same gastric symptoms that I have above detailed.

ARTHRITIS OF THE TEMPORO-MAXILLARY ARTICULATION.*

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ARTHRITIC inflammation may be of a local or constitutional character.

The former may be excited by dislocations, blows, luxations, or any lesion in neighboring parts.

In the latter by some blood poison, viz.: syphilis, rheumatism, gout, scrofula, etc., and as such must have disease medicines that are antidotes or specifics to the particular blood poison.

It is my desire, in the following cases, to call attention to my method of producing *extension* in acute inflammation of this joint from either of the above causes.

CASE I.—*Gouty arthritis of the temporo-maxillary articulation.*
A. P. B., of Hanover, N. H., 60 years of age, was brought to me by the late Prof. A. B. Crosby, M.D.

He had been a man of very robust constitution, but for the past two or three years had suffered with attacks of gout, and was now certainly an object of pity to look upon.

The gout from which he had suffered came with terrific violence into both temporo-maxillary articulations, and when he came into my office his teeth were chattering, like one in a malarial chill, from excessive irritation and spasm of the muscles of the jaw.

This caused great pressure on the inflamed articular surfaces,

*Read before the American Medical Association at Richmond, Va., May, 1881.

and gave him excruciating pain, so that he got no relief except from the effects of morphine, hypodermically administered.

The arthritis was preceded by neuralgia of the inferior maxillary nerve. On examination of the mouth, I found that his teeth had no decay in them, but some were very much worn by mastication upon the crowns, and some pulps (nerves) were exposed, and in consequence he had pulpitis, causing neuralgia that was followed by acute arthritis.

In the treatment nothing could be done with him except under the effects of morphine and an anæsthetic. On entering my office, a hypodermic dose of morphine was administered, and when under the effects of the drug, he was given nitrous oxide as an anæsthetic.

This relieved him from pain, while consciousness to some extent remained.

The pulpitis, the exciting cause of the facial neuralgia, was removed by protecting the exposed dental pulps (nerves) from the air and attrition by means of gutta-percha and an interdental splint.

The principle of the treatment of arthritis in these joints is the same as in others, differing only in the method of application.

I do not know that any extension appliance has ever been used for the relief of arthritis of this joint.

The method that I employ is as follows: In this case the patient was under the anæsthetic effect of morphine and nitrous oxide. If there is any rigidity of the muscles, cautiously force open the mouth and take an impression of either the upper or lower teeth, and a rubber splint is made from the cast to cover over all the teeth in one jaw. Upon the posterior part of this splint is made a prominence or fulcrum, (D) so that when the mouth is closed the most posterior teeth close upon it, while all the anterior teeth are left free. The next step is to take a plaster of Paris impression of the chin, and from this make a splint. (A) On each end of

the splint is made a place for fastening elastic straps (B) that pass up on each side of the head to a close-fitting skull-cap (C). See fig. 1.

When the apparatus is in place and the elastic straps tightened so as to lift the chin, then pressure is brought to bear on the fulcrum at the posterior molar tooth, and so by this means extension is made at the joints, and the inflamed surfaces within the joints are relieved from pressure; then immediate relief is experienced.

FIG. 1.



As soon as this apparatus was put on this patient his pain stopped instantly, and he took no more morphia.

He continued for a time his anti-gout remedies, and after some manipulations of the lame muscles of the jaws under electricity perfect motion was restored.

CASE 2.—*Arthritis from septic poison.* Miss Clara B., of Iowa, 21 years old, was sent to me by Dr. J. C. Hutchinson, of Brooklyn. Her history, in a few words, was as follows: Six years before had exposed dental pulps (nerves) of the inferior molars of both sides. A liberal supply of arsenic was used to de-vitalize the nerve, and this was left in many days.

Soon after teeth became very painful, jaw swelled, extending also to the submaxillary gland and tonsil of left side.

Necrosis of cellular portion of the jaw containing the teeth followed, and they were all removed with the necrosed bone. The submaxillary and tonsillar glands suppurated, and opened externally with a great discharge of pus.

With this great amount of swelling in jaw and glands, she suffered much from dyspnœa. Her vital powers became very much exhausted from want of food, and septic poison.

When she came under my care in June, 1879, she was quite anæmic and very despondent—troubled with insomnia, loss of appetite, and vomiting.

No menstruation for the past 18 months. Complained of great pain on left side of the head, at the temporo-maxillary articulation; partial ankylosis. At times there was a discharge from the ear and also into the naso-pharyngeal cavity where she had quite an abscess.

No teeth in the lower jaw, while the upper set was quite perfect.

Two large scars, as the result of the suppurating submaxillary and tonsillar glands; one about an inch and a half anterior to the ramus, and the other made by the suppurating tonsil on the anterior border of the sterno-cleido-mastoid muscle, one and a half inches long by one-half inch broad.

Treatment consisted in applying an extension apparatus in the same manner as in the first case. Her alimentation consisted in a teacupful of warm milk to which was added one teaspoonful of maltine, pepsine, and pancreatine every three hours. Vomiting stopped and digestion immediately improved, and then tonics were added. Warm sponge baths of salt water and bay rum every night and morning, and a mild current of electricity.

She continued to improve, and in Feb., 1880, menstruation returned. In March the apparatus was given up and a lower artificial denture inserted, and now her mastication was restored. In June, 1880, one year after treatment was begun, she had gained 24 lbs. in weight, and has been perfectly well up to the present time.

CASE 3.—*Arthritis from impacted wisdom-tooth.* D. L., aged 18 years, had an impacted right inferior wisdom-tooth that produced extensive swelling with partial ankylosis. Great pain in the joint.

Administered an anæsthetic, forced open the mouth, and removed the impacted tooth lying in the ramus. Extension apparatus applied; muscles were manipulated under electricity every day, and at the end of two months' treatment he was well.

CASE 4.—*Arthritis from tonsillitis.* Miss Mary H. had a severe attack of tonsillitis which extended into left temporo-maxillary articulation. Had been suffering some time before I saw her. Unable now to open her mouth. Contracture muscles very stiff from misuse.

An apparatus was worn, and every day the mouth was cautiously opened so as not to stir up any new inflammatory process in the joint. She made a good recovery.

In all cases of arthritis the muscles suffer more or less, according to the extent of the inflammatory process. Contracture muscles get stiff from the products of inflammation, while in the depressor muscles may be expected some degenerative changes from want of exercise.

I will not further trespass on your time but only say that from experience it will be found:

(1) That arthritis of this joint, like all other joints, the result of local or constitutional causes, requires proper and prompt treatment, as it may pass in a very short time from its most incipient stage to one of suppuration and destruction.

(2) That arthritis without proper treatment more often results in fibrous ankylosis, and that bony ankylosis is the exception.

(3) That the highly developed muscles of the jaw, from pathological changes, the result of inflammation, or even from misuse, have always more or less impaired motion, and in some cases require more treatment than the joint trouble.

(4) Cases do sometimes occur in which the poisonous effects of overdoses of mercury have had a disastrous result.

AN ANALYSIS OF THE PRINCIPLES ON WHICH DEPENDS THE SUCCESS OF THE MODERN METHODS OF TREATING WOUNDS.

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THE most cursory examination of the surgical literature of the past ten years cannot fail to reveal the fact that there has been a distinct change in the general principles governing the treatment of wounds. Major amputations have been usually selected as the source whence the comparative statistics of the results of different plans of treatment have been drawn. In the present paper this class of operations is had chiefly in view, although, of course, the same principles are necessarily involved in the treatment of other operation wounds. It requires but a brief analysis to perceive that all the apparently endless varieties of methods can in reality be grouped in three classes, viz. : the strict Listerian antiseptic system, the modified antiseptic method, and the open method. When the reader learns that by the first, the limb to be removed is most carefully cleansed by ordinary means, and then disinfected by carbolized water ; that an antiseptic spray never ceases to play upon it ; that all instruments are first rendered aseptic, and afterward, during the operation, never allowed to become contaminated—for when accidentally removed from the spray or

coming in contact with the table, bedding, etc., they are placed in the antiseptic bath ;—that the surgeon and his assistants must not only cleanse their hands in an ordinary, but in an extraordinary manner ; and that the admission of a single germ might be fatal to the success of the operation, he is impressed with the absolute necessity of the exclusion of uncarbolyzed air from the wounded surface. Next come the minute directions as to the closing of the wound, which is to be coaptated in the closest and most perfect manner. As, however, certain small cavities are inevitable, owing to slight irregularities of the flaps, intermuscular spaces, etc., besides the larger one left around the sawn bones, fluids must collect. As these would prove injurious, if for no other reason than that they prevent the coaptation of the flaps, some method of drainage is instituted for their removal by tube, catgut, or strands of horsehair. Finally, the wound is covered by complicated and voluminous dressings. Whenever the discharge appears on the exterior of the dressings they are renewed, the same antiseptic spray and preparations being invariably employed as at the time of operation. However sceptical the reader may be at the outset ; however he may insist he has often obtained results as good with less complicated dressings, the invariable success claimed by Lister stares him in the face. Some are inclined to evade the issue, and claim that his statistics are unreliable ; but the unanimous published experience of numerous distinguished surgeons all over the world, who have really tried his method, and the eminent recruits daily joining the ranks, must convince any fair-minded man that there is not only something, but a great deal in it. I think that the above statement is strictly true. The most bitter opponents of Listerism are among those who have either no experience of it whatever, or who have tried it in *such a way as to invite and secure failure*. Those who have faithfully

tried it seem chiefly concerned not in denying its usefulness, but in reducing its complexity, and in endeavoring to determine which of the various portions are essential, which non-essential.

The reader becomes, we will say, a convert to Listerism, and tries the dressing. He soon meets, however, with an advocate of the diametrically opposed school, viz., the "open method," who says that he uses neither disinfection nor dressing, but simply by position, etc., secures moderate coaptation of his flaps and efficient drainage, and claims that his results are as good as Lister's, at least as good as he thinks possible, forgetting that nothing short of *perfection* should be aimed at, although failure be certain, and that the measure of success which satisfied the surgeons of fifty years back, is not our present standard.

A little staggered by such a violent upheaval and disastrous overthrow of his so-recently adopted views, he remains in an unpleasant state of uncertainty, disinclined to entirely discard the conclusions he has arrived at after careful pondering and investigations of the statements, facts, etc., of the Listerians, yet with his faith shaken by the apparently irreconcilable facts presented by the able expositors of the "open method."

Perhaps in this dilemma he either arrives at the same conclusions as, or becomes acquainted with the writings of, those who pursue the "modified antiseptic method," and who equally claim with the other two schools the palm of success. Its latest teacher is von Brun, who, discarding the spray, after closing the wound and providing for efficient drainage, syringes it out thoroughly with large quantities of weak carbolized water, and then immediately dresses it antiseptically. *This dressing remains for forty-eight hours, when the wound is treated by the "open method."*

Most practitioners under this conflicting mass of testi-

mony fail to detect the true principles involved, upon which depend the undoubted success attained by all these methods. Many men are too indolent to investigate the matter closely, and so come to either one of two shallow conclusions, viz.: that each advocate is either intentionally or unwittingly dishonest in his statements, or that none of the methods in reality are of any special value, that there is no real advance in surgical practice after all, and that the old beaten paths are so much easier than the new—which besides present no corresponding advantages—that he will travel on in the ruts worn by his predecessors. I maintain that this is all wrong and unscientific, however eminent he may be who gives utterances to such sentiments. This is the natural result of the hurried and deficient training given to too many men; primarily in their preliminary education, secondarily and more fatally in their strictly medical curriculum. In support of my statement, look at the notes taken by students at lectures, mark the replies given in a quiz-class, and both will demonstrate that, through either the fault of teacher or pupil, the latter is almost totally careless, and therefore ignorant of the principles which induce his surgical teacher to recommend a certain dressing or plan of treatment, and is only eager to learn how the eminent Prof. B. treats this, that, or the other disease. The result is when he meets a case of the same affection, he treats it as Dr. B. did, utterly regardless whether the case really *presents the same indications*, only solicitous to find whether it is the *same affection*, believing it only necessary to follow authority and he is sure to gain the same results. From his frequent want of success, if an inferior man, he falls into a careless, routine, empirical, and sceptical plan of treatment, which ends apparently—as is to be expected—in confirming his views as to the equal inefficiency of all plans of treatment. If either a more sensible or su-

perior man, he sets to work to educate himself properly, and then becomes capable of discriminating successful methods, because untrammelled, although profiting by authority, he always endeavors to detect principles and causes, so as not to waste his energies upon the protean results which too often elude capture and subjection.

The human mind seems to possess an innate power of attributing mysterious causes for results which at first sight are difficult to comprehend. From this intuitive intellection results the attributing to a certain plan of surgical treatment something *sui generis*, something even unexplainable, when in reality the result is clearly due to the skilful employment of certain well-recognized principles. All this uncertainty has been, doubtless, chiefly owing to the want of accurate experimental pathological knowledge, but much of this lack has been already supplied, and more is being accomplished daily in this direction. I hope to show that for the subject treated of in this paper we already possess a fund of knowledge sufficient to explain all essential points.

I will, therefore, make certain preliminary statements, which are absolutely required for a clear understanding of the subject.

Let us first examine the condition of an amputation wound after it has been put up. The capillary and smaller vessels are sealed by clots which extend to the next collateral branches; but, of course, these same vessels, where not filled with hæmostatic plugs, are in an excellent condition to absorb anything brought into sufficiently close proximity. In a few hours, especially in women and children, where, owing to the thinness of the epidermis, the congestion is not concealed, a marked flush is seen. This increased vascularity is, of course, present in the deeper structures also. In the normal course of events this con-

gestion is relieved by a more or less copious flow of serum, which is finally substituted by pus. As early as the third day—and I think that I have observed this change even before in quite a considerable number of cases,—by the aid of a lens, granulations may be seen springing up in those portions of the wound where perfect coaptation has been impossible.

Let me recount one more set of preliminary facts experimentally proved by Billroth, viz., that an artificially prepared septic fluid can be placed in intimate contact with a *perfectly healthy granulating surface with impunity*; while another portion of the same, when injected into the cellular tissue, will rapidly destroy the animal, with the most marked septic symptoms. He also states that since in compound fractures he has adopted the use of the plaster-dressing, the occurrence of septicæmia and pyæmia has been much less frequent in his practice.* By the more perfect quietude insured by the fixed dressing, the granulations are not mechanically injured by the frequent, although slight, movements of the fragments; the muscles are maintained in a state of quietude, and consequently the pus cannot be either worked into the cellular interspaces nor into the granulations, or breaches in them produced by movements of the fragments, and injudicious handling. Now what is the most natural inference from this? Plainly, that which experience has already taught us, that perfectly healthy, well-formed granulations are an efficient barrier to the absorption of septic materials, at least for a time. I do not deny that acrid septic fluids, whether produced by the addition of a special ferment from without, or generated by bad hygienic surroundings, systemic conditions, etc., may, like caustic, rapidly destroy healthy granulations and thus allow of poisoning of the system, but I am prepared to maintain

*Viewed in the light of the above-mentioned experiment, the explanation of such good results is readily discernible.

that for a considerable time they do prevent absorption. Besides, I have been so very often told by those who should know better that a given wound was doing beautifully with a perfectly healthy granulating surface, with the qualifying statement, however, that it refused to cicatrize as it should, and upon inspection have found that portion only of the professional opinion concerning the "not healing" was correct, that I am always sceptical when I hear that a thoroughly healthy surface in all its parts immediately preceded septic trouble. It should also be borne in mind that all structures do not form granulations with equal rapidity, so that while the accessible portions of a wound may be to the eye healthy, other deeper parts may be in an exactly opposite condition.*

Now let us examine the conditions of the wound during the first forty-eight hours as to sepsis. Whether we believe that suppuration—specially unhealthy suppuration—and septic trouble are produced by the importation of germs from without or not, there can be no question that the secretions of a septic wound, when applied to solutions of continuity in other individuals, will produce a similar constitutional affection. What are the conditions most favorable to such absorption? Clearly the presence of a secreting surface capable of absorbing, and a favorable nidus for the development of the *materies morbi*, whether it be chemical or bacterial.

The study of the physical conditions favoring osmosis shows us that an over-full condition of the vascular system, by the comparative stagnation of the contained blood, retards, if it does not entirely prevent absorption. This distended condition of the vessels I have already said soon

* A careful analysis of the course of the majority of cases of blood-poisoning in all its forms, will demonstrate that the commencement of the trouble, *although unnoticed*, dates back to the first three or four days after operation or injury—indeed usually within the first forty-eight hours some *slight* symptoms appear, the gravity of which time demonstrates.

supervenes after operation, and in accordance with this fact there is a variable but distinct period before anything like marked fever shows itself. Soon, however, the overloaded vessels relieve themselves by a transudation of serum, or, perhaps more correctly, altered liquor sanguinis. This relative cessation of congestion acts in two unfavorable ways, however salutary it may in other respects prove, for it provides an abundance of a readily putrescible fluid well adapted for the propagation and development of bacteria and minute organisms, while at the same time it unloads the circulation to such an extent as, in the highest degree, to favor absorption. The normal vascularity of any part subjected to amputation is scarcely fitted for rapid absorption. The excessive primary vascularity just spoken of is still less favorable to osmosis, but the state of the blood-vessels, after their partial depletion, is such as to meet all the requirements for septic or other absorption. Soon, as we have seen, granulations commence to form, so that the risk of absorption is measurably diminished by the end of forty-eight to seventy-two hours. I would beg my readers to bear this fact in mind, as it gives the key, I believe, for explaining the success of von Brun's method.

After these preliminary explanations, let us now—bearing clearly in mind the wound-conditions just described—examine what must be the indications to fulfil to avoid the risk of sepsis. Of course, no one doubts that if every wound could be made to unite by “first intention,” or immediate union, the chances of septic infection would be almost, if not absolutely, done away with. To this end, the most accurate coaptation of the wound surfaces, with their subsequent retention, is the primary essential. The most difficult portions to appose, and keep so placed, are the deeper parts, and the only agent likely to disturb the position of the flaps, after coaptation has been secured, is the accumu-

lation of the secretions, due to some obstacle presented to their escape. Manifestly, then, the second indication is to secure free drainage. The third indication is clearly to remove inflammatory products as soon as secreted, so as to render impossible any accumulation of that putrescible fluid poured out from the cut surfaces, which affords such an admirable nidus for the development of septic poison.* This removal is of paramount importance, as is clear from our previous studies, for the first forty-eight or seventy-two hours. This indication is plainly fulfilled by free drainage. Fourthly, if possible, the secretions of wounds—which must be present to some extent—should be kept from chemical change, and rendered unfit for the generation of septic matters, by agents which, if possible, shall promote the formation of granulations. To sum up, the indications are: perfect coaptation, perfect drainage, disinfection, promotion of granulations, and rigid avoidance of anything that can chemically or mechanically injure these safeguards of nature.

Now, let us see how each one of the three typical methods of wound-dressings fulfils these indications, and whether the measure of success attained by each is not to be attributed to the degree of strictness with which they are carried out.

Lister inculcates the most perfect coaptation, drainage, and disinfection which is practicable, with the least possible manipulation, and after-dressing. Undoubtedly the carbolic acid, by coagulating the albumen in the tissues, forms a superficial, antiseptic, comparatively impermeable film, which, like that formed by chloride of zinc, remains until, by the formation of granulation, the slough—for such in truth it is—becomes separated. The antiseptic method certainly renders the wound-fluids unfit for the propagation

* Experiments have proved that pressure favors absorption, so that this accumulation is dangerous on this account also.

of the septic poison; whether by preventing the formation of minute organisms, or not, is beside our present purpose to consider.

On the other hand, the "open method" succeeds by very dissimilar methods in fulfilling some of the indications perfectly, while others it professedly and necessarily does not attempt. I say necessarily, because their fulfilment would inevitably prevent the carrying out of the others.

Let us examine this method, as we have just done Listerism. The first indication, viz., as perfect coaptation as possible, is of necessity neglected, so that whatever advantage accrues from healing by primary union, is lost. In return for this, its advocates claim—and rightly—that the coaptation secured, although imperfect, is not disturbed by the accumulation of the discharges, and that they are prevented from remaining in contact with an absorbing surface for any considerable length of time, or in any quantity. By this removal, as soon as effused, of the wound-fluids, they maintain that chemical and septic changes are very unlikely, and should they obtain, do no harm, because the poison cannot remain in contact with an absorbing surface sufficiently long.

I admit that the second and third indications, which amount to free drainage to prevent separation of wound surfaces, and free drainage to obviate the risk of absorption by the long retention of the wound-fluids in contact with absorbing surfaces, are admirably fulfilled, but the fourth indication is certainly not fulfilled, other than negatively, if the strictly open method is used. The disadvantages inherent to this plan will be presently adverted to; its advantages are clear and unmistakable, but are not dependent upon anything but the fulfilment of the principles pointed out, and upon which also depends the success of the antiseptic method.

Finally, there remains for consideration the plan lately advocated by von Brun. Dispensing with the spray and the other elaborate formalities of the Lister operation, after its completion the stump is syringed out with large amounts, say half-a-gallon of weak carbolized water; it is then dressed antiseptically, and left undisturbed for forty-eight hours. The dressings are then removed, and the stump treated by the open method. Subjected to the same critical analysis as the other two systems, we find that the most perfect coaptation possible, drainage by tubes, etc., prevention of chemical or other changes in the fluids, and treatment of the wound surfaces by a coagulating agent, are relied upon, *and that this antiseptic condition is kept up for the first forty-eight hours*, in other words, until most of the primary danger is passed; because, as we have seen, healthy granulations are a protection against septic absorption, and their formation has commenced, and, in many cases, is well advanced by the time specified. At the end of the first forty-eight hours the troublesome details of the Lister antiseptic dressing are dispensed with, and the open method substituted. It is hardly necessary to point out that most of the good to be obtained by a Lister dressing is here combined with the advantages of the open method. In this plan, too, there is nothing mysterious, nothing occult, but the simple successful employment of certain well-established surgical principles.

Look at the results of Alanson's famous series of major amputations performed nearly a century ago. He coaptated the deep surfaces of the flaps by compresses, leaving the edges comparatively to themselves, thus securing the inestimable advantages of quick healing, free drainage, and—because nothing could accumulate in amount—freedom from septic absorption. Examine the records of Callender's admirable series of amputations, and upon what did his suc-

cess depend? Was it not upon free drainage, absolute quiet of the limb, treatment of the wound surfaces with a solution of chloride of zinc, thus forming an impenetrable antiseptic slough, which, upon separating some days after the operation, left a healthy granulating surface, and, finally, upon rigid avoidance of anything that could injure the granulations, merely cleansing them with a camel's-hair pencil dipped—and kept in the intervals—in an antiseptic solution? The limb, where opportunities served, was swung for some days before the operation, which, of course, rendered the patient accustomed to the new position. This, in part, obviated those movements which Billroth has shown are apt mechanically to work the pus into the intermuscular spaces, where it can be absorbed, since it must then have broken down the barrier of the granulations.

I maintain that the success of all these definite methods of wound-dressings,—indeed, the good result achieved by every surgeon, is necessarily dependent upon the degree in which he succeeds, wittingly or unwittingly, in fulfilling the indications pointed out in this paper.

Having been the first in this city, as I believe, to perform a Lister operation, so early indeed as to have had to manufacture catgut, protective, gauze, etc., myself, I think that I am in a position to at least speak intelligently upon the subject. As to the “open method,” I have practised it, as hereafter described, in quite a number of cases. Von Brun's modification of both I have been using exclusively for the past few months in all cases where feasible, notably in nine amputations.

Provided Lister's directions are properly carried out, his method, though troublesome, seems to me both theoretically and practically the best. To operate satisfactorily it demands skilled assistance, some experience, and the utmost attention to detail combined with a thorough ap-

preciation of the principles involved. By excessive care upon the part of the operator, skilled assistance may be dispensed with, although it renders the labor greater, and risks the success of the operation. It is utterly unfair to deliberately transgress those rules upon which Mr. Lister lays most stress as being essential to success, and then condemn his method. He distinctly says that carbolic acid is an irritant to the tissues, and, therefore, by a special protective specially tested, he keeps it from direct contact with the wound; otherwise suppuration will occur. As the spray itself is an irritant—any operator's hands will demonstrate this,—special provision for draining off the excessive amount of serum poured out must be provided; yet, surgeons will almost hermetically seal a wound by stitches, straps, etc., thereby rendering suppuration from tension certain, when Mr. Lister has said that a single stitch producing tension will provoke suppuration. In this latter case, however, the pus does not become offensive.* Again, the less of the direct spray coming in contact with the raw surfaces the better, and yet we see a powerful atomizer within a couple of feet of an open abdominal cavity, the spray playing directly upon the exposed peritoneum, cooling it, if not doing other fatal damage, when the cry is again raised that Listerism is worse than useless. But what I have just described is *not* Listerism. The magnificent triumphs in recent abdominal surgery, considered by the most noted operators to be due to the use of this method, plainly demonstrate what the method in proper hands is capable of. An ovariectomy can be usually done with impunity, with strict Listerism, in a general hospital, where, without it, certain

* During the hottest summer weather I have dressed a bad compound comminuted fracture of the leg, where profuse suppuration had resulted from inadequate drainage, at intervals of from *four to eight* days, without the faintest trace of putrefactive odor being perceptible. Other similar cases could be cited which have been treated by the author, but this will suffice to prove the truth of my statements.

death has been proven to be the almost inevitable result. I do not advise such temerity, of course, still it is constantly successfully done. This is not the place for me to demonstrate the proper method of operating, but, as far as my experience goes at present, Lister's dressing is undoubtedly the best for operation wounds.

My readers will say, all this care and experience are not possible to the average practitioner, so that the method is too complex for general use. Even were this true, no one has the right to withhold any possible source of safety from his patients on any such grounds as the above. Besides, a little study, and a resolute adherence to details, will soon render one expert; indeed, every thing may be perfectly done at the first operation, *with care*. At least you need not, after dressing a profusely suppurating wound, carelessly wash your hands with a little soap and water, and then expect your antiseptics in a Lister operation to prevent trouble; you need not deliberately lay your instruments and sponges upon hospital blankets, and operating-table in use for years, or on the floor, and then complain that you have been deceived in your expectations. Having been often annoyed in this way, I am merely detailing my actual personal experience, and that is why I consider skilled assistants so desirable, who certainly would never be guilty of such fatal errors.

The great objection to the "open method" strictly applied, is the risk of accidental contamination by pus globules, etc., in the dust and air coming into contact with the wound. Again, union by the first intention not being aimed at, two results necessarily follow, viz.: the risk of retraction and melting down of the flaps, and a much more extensive cicatrix than by the usual methods, which in amputation renders the stump often troublesome. These are not theoretical but practical points. If the stump has

loosely laid over it a fold of lint wet with some antiseptic fluid, thereby preventing accidental contamination, and the formation of scabs which prevent the free egress of the discharges, the first objection is largely removed, but the others remain. With the most favorable surroundings, admirable results have been obtained by this method, as far as low mortality goes.

Finally, von Brun's method remains for consideration. The omission of the spray at the operation, with the immersion of the instruments in the antiseptic bath, are all that is gained over Listerism at the outset. It tends, moreover, to promote carelessness in little points of cleanliness, the importance of which has been gradually gaining ground in the minds of the profession, by apparently demonstrating that they are unnecessary, because a deluge of carbolized water will set all right again. This may be true, but some time it will *not* be true and a life may be sacrificed. As has been shown, the most important advantages of antiseptic dressings have been secured by the time the open method is resorted to. The extent of the objections to the open method which is then substituted, is lessened to the exact degree to which the healing has progressed by that time. By protecting the wounds, as I have suggested, much additional evil is obviated.

In my judgment, next to the skilful use of the Lister method, von Brun's is by far the best at present known to surgeons. By it I have gained exceptionally good results, but, as I hope I have shown, it fails in *continuing* to fulfil the indications as perfectly as Listerism does.

In conclusion, I can only express the hope that I have succeeded in rendering clear a subject which to many practitioners seems to be surrounded by a haze of uncertainty. There is nothing, I repeat, mysterious about the successful treatment of wounds, operative or otherwise. You must

treat them scientifically, which is but another word for successfully. It is essential to recognize general principles and indications. The subject is far from being exhausted, but space forbids further expansion. Let no one suppose that I have more than just lightly touched upon the application of Listerism or the other methods, although I, have, I trust, treated sufficiently of the principles involved. If I have contributed toward popularizing any of the three methods mentioned, especially Listerism and von Brun's, I shall have abundant cause for self-congratulation. I hope to see the time come when medical men shall all be so educated, or have the opportunity to be so educated, as to render papers like the present totally unnecessary, since all then grasping the importance of general principles, and accustomed to treat such subjects logically and scientifically, will need no one to point the way, but will themselves be industrious multipliers of our knowledge and experience.

THERAPEUTIC CONTRIBUTIONS.*

II

HYOSCYAMIA AS A DEPRESSO-MOTOR.

By E. C. SEGUIN, M.D.

IN the preceding paper I have presented a *résumé* of the state of our knowledge on the subject of the hypnotic action of hyoscyamia, together with the experience of the Committee on Neurotics of the New York Therapeutical Society. The following statements are with reference to the almost equally important depresso-motor or paralyzing effect of the drug.

It may be well to premise that in 1869, Dr. John Harley wrote: "In convulsive affections, it (hyoscyamus) has proved useless in my practice. The plant undoubtedly exercises a considerable depressing influence on the corpora striata, but it fails to diminish the excitability of the spinal centres, if it does not actually exalt it."† Harley, however, speaks of hyoscyamia as valuable in cardiac functional disorders, in neuralgia, especially visceral, in cardiac and pulmonary asthma. Oulmont‡ was more fortunate in his clinical experience. He employed Merck's crystallized hyoscyamia, in doses from .001 to .003 several times a day,

* No. 1 of these contributions appeared in the April number of the ARCHIVES, under the title, "Report on the Use of Hyoscyamia as an Hypnotic and Depresso-Motor."

† "The Old Vegetable Neurotics," London, 1869, p. 340.

‡ *Bulletin de Thérapeutique*, tome lxxxx, p. 481.

in pillular form, or hypodermically, and gave enough to obtain full physiological effects. He treated several cases of neuralgia, occipito-cervical, sciatic, etc., and nearly all the cases were cured pretty rapidly. In mercurial tremor, even in cases which had resisted sulphur baths and iodide of potassium, he obtained four cures and two reliefs. A case of progressive locomotor ataxia was not favorably influenced by hypodermic injections of hyoscyamia, excepting that the pains were somewhat dulled. A severe and rapidly fatal case of traumatic tetanus received doses which Oulmont considers to have been too small; the pain of the spasms was moderate, but the tetanus remained unchanged (this is in accord with Harley's statements). He had no opportunity of treating paralysis agitans, though strange to say he is often referred to as having been the first to use H. in that disease; he merely refers to Charcot's experience.

This trial of H. by Charcot is more explicitly stated by Ordenstein in his essay on paralysis agitans and sclerosis.* He says: "We have yet to mention a last therapeutic experiment. M. Charcot has recently given [to cases of paralysis agitans] two or three granules of about .001 of hyoscyamia per diem. This medicine has procured several hours of rest to a number of patients. New observations are requisite to enable us to express an opinion as to the value of this treatment."

Dr. Lawson† thought that H. had proved serviceable in chorea and locomotor ataxia.

My own experience has been as follows:

H. IN PARALYSIS AGITANS.

CASE—Mr. F., aged 42 years, consulted me, at Dr. Blumenthal's request, on May 19, 1879, and gave the following history:

* Ordenstein, *Sur la paralysie agitante et la sclérose en plaques*, p. 31. Paris, 1868.

† West Riding Lunatic Asylum Reports, vol. v, 1875.

Good health until 1874, when he had a severe attack of "inflammation of the bowels" lasting for several weeks. A fistula in ano formed not long afterward and has continued. Without any other apparent cause he began to tremble in 1875, the fingers of the left hand being first affected. Not long afterward the left leg became affected, and in the course of two years, very gradually, the right hand and arm became involved. The symptom has steadily increased in intensity in both upper extremities; the left leg is about as it was three years ago, and the right lower extremity is unaffected. The legs feel weak, but patient has noticed neither pro- nor retro-pulsion. No numbness of limbs, headache, dizziness, loss of memory, or affection of general health. Parents did not tremble.

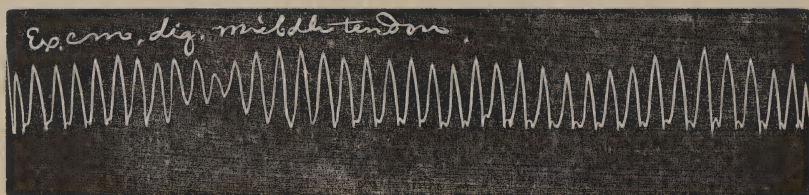


FIG. 1.

Tracing of movements of middle tendon of the extensor communis digitorum, in paralysis agitans.

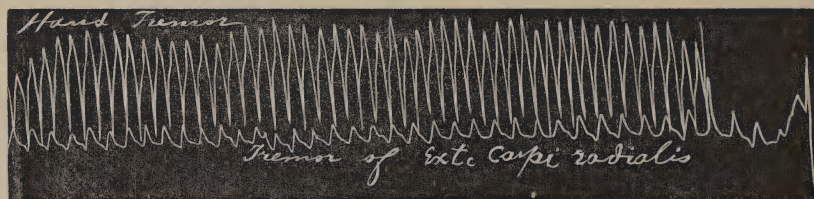


FIG. 2.

Upper tracing shows regular flexion and extension movements of the hand; the lower, pulse-like tracing represents the tremor of the extensor carpi radialis longior taken on its tendon; paralysis agitans.

Examination.—General condition excellent. Facies is injected and exhibits the characteristic stare of the disease. No anæsthesia of face or hands. Dynamometer test of strength of grasp made useless by injury to right hand; no apparent loss of power. The tremor affects the left leg (slightly) and the two upper extremities; the head and right leg are perfectly still. The movement is constant, consisting of alternate movements of flexion and extension (mostly) made with great rapidity and with absolute rhythm.

The rhythmic nature of the trembling is well shown in the annexed graphic tracings. Emotion makes the tremor worse; it can be temporarily checked by the will, and coördination of movements is exact; *i. e.*, there is no ataxia; he can place a finger on his nose with closed eyes, and can carry a full glass of water to his lips. The hands when resting naturally on the knees make the well-known pawing movements. The normal use of the hand for eating, dressing, etc., is much hampered by the tremor and by a certain slowness and stiffness in willed muscular movements. The patellar tendon reflex is normal, or perhaps sub-normal.

It may not be uninteresting to add that Mr. F. has been under the care of several eminent specialists for diseases of the nervous system, two of whom had looked upon the disease as sclerosis of the brain and cord. (This is a good illustration of the foggy notions which prevail as to the meaning of the forms of tremor and the diagnosis of disseminated sclerosis.) He had attempted a great variety of treatment, perseveringly, without relief, by means of, for example, sedatives, counter-irritants to the spine, hypodermic injections of strychnia, etc.

I told the patient frankly that nothing would cure him, but proposed a trial of hyoscyamia as a means of relief. The drug was administered for several weeks, hypodermically, and the following are notes of effects observed.

On May 21, 1879, I prescribed :

R

Hyoscyamiæ,	gr. i.
(Merck's crystallized)	
Glycerinæ,	
Aquæ destillatæ,	āā ℥. C.
Acid. carbol. pur.,	gtt. ss.

Mix, filter with care, and label :

"Hyoscyamia solution for hypodermic use, ℥. i = $\frac{1}{200}$ grain."

This prescription was dispensed by Messrs. Caswell & Hazard, and in the afternoon of the same day I gave the patient an injection of ℥. iv ($\frac{1}{200}$ grain) in the arm. In the course of half an hour *all tremor had ceased*, the mouth was parched, and the pupils somewhat dilated. In an hour after the injection, the patient was greatly distressed by dilatation of the pupils and dimness of vision, by extreme dryness of the mouth causing almost complete aphonia; and there was slight delirium. Four hours after the injection these symptoms had in a great measure passed away, *but the*

hands were still absolutely quiet, and the tremor only very gradually reappeared during the ensuing two hours.

Thus $\frac{1}{80}$ grain of hyoscyamia produced a complete cessation of the tremor of paralysis for a period of at least four hours; a result which was as surprising as it was gratifying to me, whose experience had been that no means known to our art, short of complete anæsthesia, were capable of arresting this trembling.

On subsequent days I injected from three to four minims, with invariably the same results. In a week or two a certain degree of tolerance was established, and three and a half or four minims of the solution did not produce much distress, but still suspended the movements of the paralysis agitans for two or four hours.

The patient, and his relatives also, noticed that going up and down stairs, using hands for buttoning his clothing, feeding himself, etc., became much easier, and he felt stronger.

While the daily hypodermic injection of hyoscyamia, once a day, thus caused very great temporary relief, and produced a certain amount of positive continuous improvement, there were no unpleasant effects of any importance. The digestive organs remained in good condition, the accommodation was not wholly paralyzed, no abscess or special irritation was produced by the injections.

Early in July, pills containing $\frac{1}{80}$ grain (.002) of hyoscyamia were substituted for the injections, with equally happy effect; two pills a day giving the patient *several hours of absolute freedom from tremor*, without unduly severe toxic symptoms. During the month of August, the patient became more sensitive to the drug, and the doses were reduced to .002 in the morning, and .001 in the afternoon. The paresis of accommodation varied somewhat from time to time, but was easily corrected by weak convex glasses. Prof. H. Knapp was consulted upon the question whether permanent injury to the eye might result from the long-continued or indefinite use of the medicine, and he replied that it could not.

The patient returned to his home in Louisiana, in September, taking with him a number of pills made at my request by Messrs. McKesson & Robbins, each containing $\frac{1}{80}$ grain of hyoscyamia.

I have since indirectly heard that the disease has made progress.

I have used hyoscyamia in several other cases of paralysis agitans, and always with the same effect, viz., temporary

relief. The drug has been given in these other cases by the mouth, using the tablets of $\frac{1}{50}$ grain, made by Messrs. Caswell & Hazard. One tablet in the forenoon and one at bed-time have been sufficient to almost arrest the trembling for a period of time varying from one to three hours, and to cause a delusive sense of improvement throughout the period of treatment. After a few weeks, on omitting the drug, it was observed that the tremor was as before. Indeed, I should add that in all my cases the usual progressive development of the disease has not been prevented.

Still it is a real addition to our therapeutics to have a drug which can, for a few hours, afford physical rest to a patient tormented by paralysis agitans. The hypodermic injection of .0006 to .001 ($\frac{1}{100}$ to $\frac{1}{80}$ grain) will surely do this; and the internal administration of from .001 to .002 will produce a similar less-prolonged suspension of tremor.

H. IN CHOREIFORM AFFECTIONS.

CASE. I.—Coördinated rhythmical hysterical spasm (hammering or pounding chorea).

Mrs. K., aged about 28 years, was placed under my care during the latter part of June, 1879. I learned that she had been hysterical for many months; having weeping, globus hystericus, at times pseudo-coma with rigidity, and nearly constantly of late a peculiar spasmodic affection which will be described in detail. This spasmodic movement was at the time I saw the patient almost the only symptom present. Appearing late in the winter, it had changed its character and rhythm a good deal, but had never been completely suspended. Mrs. K. had suffered from no corporeal disease, except what, from the imperfect account I received of it, may have been imperforate hymen, which had been successfully operated during the spring by a prominent gynecologist.

When I first saw Mrs. K., she was sitting up in bed, looking pale, but not much emaciated; her eyes were bright, and she was perfectly clear-minded and reasonably calm. A pillow lay upon her knees, and she was pounding it with her two closed fists,

with a regular up-and-down stroke. The blows were quite hard, given with perfect rhythm, both hands coming down together, at the rate of one hundred strokes per minute on the average. There was no marked motion above the elbows ; it was a regular hammering or pounding. The excitement of the examination momentarily increased the rate of hammering, and this also occurred on one side when the other hand was forcibly held by the observer or kept still by volition. The will could for a few moments suspend the movements, but the effort caused distress, and was followed by increased rapidity and force of striking. The noise made by the pounding could be heard a long distance, and on several occasions, when the pillow had slipped out of position, Mrs. K. had bruised her knuckles.

I was informed that this extraordinary performance was kept up all day and evening, until sleep supervened. The trained nurse in attendance estimated that the pounding was done for fourteen hours on some days, and eighteen hours on others. A simple calculation will show that from eight to ten thousand double blows were struck each day by this lady. There was no apparent fatigue from this wonderful expenditure of motor force ; and several times visiting the patient late in the evening I found her nearly as fresh-looking and as amiable as in the morning. She was tastefully dressed nearly the whole day, but sat on a lounge and occasionally took a few steps around the room, hammering all the time.

Occasionally, almost each day, the hammering ceased, and was replaced by tetanic rigidity of the extremities and trunk, with or without seeming coma ; sometimes with emotional excitement.

I made a thorough examination of the patient, except, owing to the patient's refusal, of the sexual organs, with a negative result. The urine was especially examined and found free from signs of renal disease.

The case after making marked progress in relation to the hysteria and the special spasms, terminated in a most tragic manner during my absence from New York one Sunday, by uræmic convulsions and coma. A *post mortem* study of the urine showed albumen, hyaline and granular casts.

I desire to report the effect of hyoscyamia upon this choreiform affection, premising that no medicine tried by the several physicians who had treated the case before me, had arrested the hammering.

The solution employed was that for which a formula is given

on p. 283. On June 28th, at 1 P.M., I injected M. ii ($\frac{1}{100}$ grain) of the solution under the skin. At 1.40 there was marked flushing of the face, dilatation of the pupils, dryness of the mouth; the pulse unaffected, beating sixty-five per minute. *The hammering was reduced to the slightest blows*, given with great regularity. This reduction of the spasm occurred very gradually, from very hard pounding at the rate of more than one hundred blows per minute. The patient complained of a sense of weakness in the arms, and of drowsiness, and fell asleep for three hours. The pounding did not reappear until some time after waking, and then it was weak. At 9 o'clock P.M., I made a second visit, and found the patient comfortable—pounding very gently. I then injected a little less than one minim (about .0005 or $\frac{1}{200}$ grain) with the same gratifying results, viz., moderate toxic effects, and complete cessation of spasm with good sleep.

These effects were regularly obtained each day for a fortnight, but at the end of that time it became evident that the effect of hyoscyamia was only palliative, as in paralysis agitans.

CASE 2.—Chronic chorea in the adult. Male, aged 45 years, employed as a helper at a railway depot for the past twelve or fourteen years, has been the subject of most marked general chorea. During these years he has had choreic movements affecting the extremities, neck, face, tongue, jaws, etc.,—in brief, the whole body. In the last few years sleep has been greatly interfered with by the continuance of movements at night. Even when asleep he has jerked about in bed to such an extent, striking out with his arms and legs, and grating his teeth, that his wife had been unable to occupy the same bed.

Examination showed only common chorea. No paralysis, anæsthesia, or other signs of organic disease of the nervous system. Speech affected as in common chorea, not as in sclerosis. Facies and manner somewhat demented. Intelligence fairly preserved. Examination is made difficult by extreme choreic disorder.

January 7, 1880, he received $\frac{1}{100}$ grain (.0006) of hyoscyamia, hypodermically at 9 P.M. The only effects were considerable dryness of the mouth and some incoherence in the night.

Jan. 8th. At 10:10 A.M., pulse, 74; axillary temperature, 36.5°C. ; respiration, 20; pupils, 4 mm. diameter. Can hardly keep feet and hands still an instant. The mouth and tongue are in such constant motion that it is impossible to take temperature in the mouth. Administered $\frac{1}{80}$ grain (.0008) of hyoscyamia. At 11:10

A.M. is sound asleep and perfectly quiet; a state of things not seen in the last eight years. Pulse, 82; pupils, contracted. After being awakened he remained fairly quiet, and would protrude the tongue, a thing impossible before. Pulse then, 90; skin, warm; and axillary temp., 36.7°C . Pupils, wider. From this time on, the administration of $\frac{1}{30}$ grain (.0015) morning and evening kept the chorea almost completely in abeyance, and procured good sleep. The drug began to affect him in about half an hour, and in two hours a maximum effect was reached.

Later, for a day or two, the dose was increased to .002 and even .003 ($\frac{1}{30}$ and $\frac{1}{20}$ grain). The chorea was completely suspended by these doses, but excessive dryness of the mouth and nausea were produced.

Once the treatment was suspended for 48 hours. The chorea returned to a moderate extent. Later, the use of hyoscyamia was wholly suspended, partly because of nausea and also because of the addition of Fowler's solution to the treatment. The arsenic was given hypodermically twice a day in doses of 10 minims finally. Some toxic effects were manifested. About March 1st the patient returned to his home in the country decidedly improved, and was able to resume work. He was seen in the middle of March, 1881, and proved to be very much less choreic than when treatment was instituted.

During the course of the treatment by means of hyoscyamia a number of observations on the pulse, respiration, and temperature were made with the same result as on the first two days. Two experiments were also made with the view of determining whether the patellar tendon reflex was affected by the drug. On February 13th at 9 A.M. the reflex was found normal. Administered $\frac{1}{30}$ grain (.002) hyoscyamia under the skin. In half an hour there was great dryness of the mouth, dizziness, staggering, and sleepiness. Went to bed and slept soundly for two hours without a movement. Awoke, talked in a flighty manner, and went to sleep again. Was seen at 2 P.M., still moderately under the influence of the drug; tendon reflex at knees, normal. At 5 o'clock P.M. reflex, normal. Gave .002 again, with full physiological effects. In this condition, extreme dryness of mouth, dizziness, staggering gait, hallucination of hearing (heard a cat), great congestion of face and neck, violent headache, and dilated pupils; knee reflex remained normal.

I am indebted to Dr. R. W. Amidon for the details of this observation.

In many of the reported cases of mania treated by means of hyoscyamia, whether given by the mouth or hypodermically, muscular relaxation is mentioned as a prominent symptom following a full dose. (*Vide* cases by Dr. Shaw in April number of ARCHIVES.)

These cases demonstrate, it seems to me, that hyoscyamia is a powerful and constant depresso-motor or anti-spasmodic. The spasmodic movements of paralysis agitans, of hysteria, of chorea, can be completely arrested by it for several hours. The power of retaining the erect or sitting posture may also be annulled by hyoscyamia. These facts and Dr. Amidon's observations on the state of the tendon reflex during the toxic state produced by hyoscyamia would seem to justify the conclusion of Dr. John Harley, cited at the opening of this paper, that the "plant undoubtedly exercises a considerable depressing influence on the corpora striata, but it fails to diminish the excitability of the spinal centres, if it does not actually exalt it."

The experience of all observers, with reference to the physiological and therapeutic effects of hyoscyamia, may be summed up as follows:

1. It acts as a mydriatic, but whether more fully or longer than atropia remains to be settled.

2. When given in small doses it reduces the cardiac pulsations, increases arterial tension, and checks the loss of body-heat. It also produces hallucinations and delirium. It may cause a fall of axillary temperature, and occasionally a rash.

3. In large doses it immediately increases the pulse-rate, produces a seeming paralysis or motor debility, and sleep.

4. Hyoscyamia is indicated in mania, restlessness, delusions of persecution, dementia with agitation and destructiveness, epileptic mania, insomnia, rapid action of the heart,

epilepsy (?) status epilepticus, chorea, paralysis agitans, hysterical spasms, tremor, neuralgia, rapid pulse, etc.

5. In mania and allied states it produces sleep as certainly, or even more certainly, than chloral, without any evil after-effect, unless it be occasional gastric disorder.

6. In cases of delusions of persecution or of suspicion it has produced a positive cure.

7. In paralysis agitans it achieves what no other remedy ever has done, viz. arrests the movements for four hours or more without insensibility.

8. In the status epilepticus it shortens the attack materially; perhaps better than any other single remedy.

9. It is a diuretic of no mean power.

10. The curative power of hyoscyamia does not appear to be great. In some cases of insanity its use has been followed by recovery, but, as a rule, we must look upon it as a good narcotic, often speedier, more complete, and less objectionable than morphia and chloral hydrate.

11. In spasmodic diseases, so far, we can speak of hyoscyamia only as an ameliorating agent, or as a palliative.

I would suggest, lastly, that in cases of very acute chorea, where death is threatened by incessant motion, hyoscyamia, given hypodermically, may prove of benefit by securing muscular relaxation with certainty, thus allowing the patient to rest, and giving time for other remedies to act.

EDITORIAL DEPARTMENT.

HIGHER MEDICAL EDUCATION IN NEW YORK.

II.

In the editorial article which appeared in the April number of the ARCHIVES, an attempt was made to show that the present medical organization of our general hospitals was unfavorable to their inmates, was not calculated to favor the cultivation of medical science, and was not such as to afford instruction of a higher kind to the internes and to physicians who chose to follow the hospital service.

The chief evils in the medical organization of our hospitals were stated to be the comparatively small number of resident pupils or internes, the excessive number and unequal capacity of attending or visiting medical officers, and, above all, the short periods during which they are on duty.

It is always more easy to criticise and condemn than to build up, and I am not as sure of the value of the remedies which I shall propose, as I was of the unsatisfactory nature of the system I portrayed.

1. The attending or visiting medical staff should be made smaller in all hospitals. That this proposition is not premature is shown by the fact that the governors of the New York Hospital, and the managers of the Roosevelt Hospital, have resolved to allow the number of physicians and surgeons in their institutions to fall, by natural means, to four of each class.

In general terms, the visiting staff might be so reduced as to allot between thirty and forty beds to each physician or surgeon. Upon the basis of thirty beds to each service, the following would be the number of visiting medical officers in each of our large hospitals, given in approximate figures :

Hospital.	Beds.	Visiting Staff.
Charity	1,000	33
Bellevue	800	26
Presbyterian	100	3
St. Luke's	209	6
New York	150	5
St. Vincent's	250	8
St. Francis'	200	6
German	90	3
Mt. Sinai	160	5
Roosevelt	180	6

I omit from this table the Woman's Hospital and the Nursery and Child's Hospital, because they are, in all essential particulars, special hospitals.

The larger hospitals, as Bellevue and Charity, might well be organized on the basis of one physician to sixty beds, giving the former a staff of thirteen, the latter one of sixteen.

On the other hand, the smaller hospitals, as the German, might need a larger staff, say of four or five members.

2. These physicians and surgeons should be elected by the authorities of the hospitals, solely with respect to their professional excellence and reputation, and in such a way as to secure for the hospital the services of men able to carry out quasi-special work. For example, if two surgeons were to be selected, one should be a good general surgeon and the other one versed in genito-urinary surgery, syphilis, and dermatology. If three or four physicians were to be chosen, one should excel in pulmonary and cardiac diseases, another in visceral and constitutional affections generally, another in diseases of women, and, lastly, one should be expert in nervous diseases.

This brings us to the consideration of the appointment of specialists to hospitals. In the first place, in the present state of

hospital organization in New York, no general hospital needs the services of an oculist or aurist, except as consulting surgeon, because there are well-appointed institutions for the reception of eye and ear cases. I am not in favor of the appointment of specialists, or quasi-specialists, to duty *as specialists*, *i. e.*, with any title indicating what branch of medicine they prefer. I would favor the appointment only of visiting physicians and surgeons, leaving it partly to the medical staff to arrange their services to suit their tastes. The separation of cases in the hospital should, it seems to me, be allowed to take place by a process somewhat like natural selection. For example: Dr. A. being well known as an expert in digestive disorders, cases of this class might be allowed to go into his service, without there being any arbitrary, absolute rule about it. Dr. A.'s advice might, for example, be also sought by a patient with nervous disease, and there need not be any red-tape rule to prevent this. People applying at the hospital would learn in time the peculiar fitness of the various physicians; in case of doubt, members of the resident staff might be entrusted with a preliminary classification of the applicants.

It may be objected that the managers of hospitals would be greatly embarrassed in choosing men in the way suggested; *i. e.*, to select men who were eminent, or, what is perhaps better, who bid fair to become eminent in certain departments (not to use the word *specialties*) of medicine and surgery. This does not seem to me a valid objection, because it is well known that such a selection is constantly being made by people as intelligent, or even less intelligent, than are managers of hospitals, for the purpose of obtaining advice and treatment for themselves. It is becoming a more and more popular idea, or a better understood idea, that all physicians are not equally excellent in all departments of medicine; and by the help of common report, or by a few inquiries, an educated man goes quite accurately to the proper physician or surgeon, specialist or quasi-specialist, for advice. The same common-sense which prevents a woman with uterine disease from consulting a specialist for pulmonary

diseases or for nervous diseases, which leads a man with stricture to seek the help of one of a comparatively small circle of surgeons, would enable the authorities of a hospital, with the advice of their medical staff, to pick out the men who would be well calculated to develop and bring to a high degree of efficiency the various services of the hospital.

The question as to how the selection should be made, whether, as now, upon the recommendation of the medical board and by election by the managers, or by universal competition with a rigid public examination, is a very important one, and one which presents strong points on its various aspects.

The advantages of the competitive plan are well known. By it men are secured simply on the basis of excellence in passing through a series of tests; more self-possessed and clear-headed men, men qualified to become good teachers, are likely to be the successful candidates. The frequent personal bias of managers, and even of physicians, in the choice of a new member of the staff, is thus avoided.

On the other hand, competitive examinations present some important objectionable features. By them the standard of excellence is made to consist in answering questions well. Now, it is an universally admitted fact, that men who have learned the most, whose memories are best, are not by any means always the men who possess the sound judgment, the courage, and, above all, the originality which are needed to make a successful practitioner and teacher. This is borne out by the almost complete scientific sterility and professional mediocrity of naval and army medical staffs, which are filled up under a competitive examination of a high order of thoroughness. Still, by making the examination consist largely of practical tests, this objection might to a certain extent be remedied.

Selection in the way which now obtains in our hospitals presents advantages and disadvantages. Against it may be urged the undeniable fact that managers are sometimes led to a choice by extra-professional considerations, such as social influence, questions of race, religion, etc. On the other hand, if the medical

board were instructed to present two candidates for the vacant place, in order of merit, without "influence" from the managers, it is quite probable that a good man would be elected, *i. e.*, a man peculiarly fitted to take charge of the kind of cases composing the service which needed a new head.

On the whole, in the present state of medical education in New York, I am in favor of the latter way of selecting visiting physicians and surgeons, providing the spirit of the regulations be adhered to.

3. The visiting physicians and surgeons selected as above suggested, with reference to their special qualifications to take charge of wards containing certain classes of disease, should be assigned to continuous duty in the same service, and required to make daily visits at a fixed hour.

The thirty or fifty beds referred to *supra* as constituting a "service" would afford ample scope, if not filled up with incurable cases, for the display of the greatest interest in observation and skill in treatment by the visiting physician, and, would afford the internes enough material for instruction. No physician or surgeon, I may venture to assert, who intends to do justice to his hospital patients, giving each sufferer plenty of time, and to thoroughly understand all his cases; who could and would build up an experience out of his hospital work; or who designs to become a clinical teacher in certain directions, will consider a service of thirty or fifty beds as too small. Having been a personal witness to the neglect or insufficient care of one hundred patients by visiting physicians in general hospitals, and having had charge myself of services varying from twenty-five to one hundred beds, I can state quite positively that a permanent service of from thirty to fifty beds is fully large enough to meet the chief end and purpose of hospital organization, *viz.*, the cure or relief of the largest number of patients.

Of the greatest importance is continuity of service, year after year, until a good reason obtains for changing to another service or to another hospital. By having a continuous service, the physician or surgeon secures numerous advantages:

He has ample time to study his cases, and learning all about new patients is a comparatively easy task.

He can train and educate his staff of internes in accordance with his own conceptions of medical practice, and he can have a routine or traditional way of doing things established, and transmitted through the successive members of the house staff.

He is enabled to systematize certain methods of treatment, more especially for subacute and chronic cases, treatment which may extend over periods of weeks or months, and secure their execution by internes and nurses in an exact manner, year after year in the same way, to the great advantage of patients. He can at all times have a certain number of cases illustrating those diseases which he understands best, and about which he is capable of teaching students and practitioners.

He can collect numbers of well-recorded histories of cases with cure or with *post-mortem* examinations, and make these the basis of articles or books which shall be instructive to the profession, and thus indirectly profitable to the multitude of sick persons elsewhere.

The only objection which I have ever heard addressed against a continuous service, is the fact that it would interfere somewhat with private practice, and would cause the physician a loss of perhaps several hundred dollars a year. But it seems to me that a hospital physician who, from philanthropic and scientific motives, does not value his public service more than he does the adding of two or three visits a day to his receipts is not worthy of his position; and I know the profession well enough to think that managers of hospitals would have no trouble in finding candidates who would look upon the matter in a proper light. Having a hospital service is a high honor; it gives an opportunity for doing some good in an immediate humanitarian way, and in a less direct scientific manner; and a physician should be glad to give from one to three hours a day for this privilege. If there be any men who want hospital services for the nominal honor the post affords—to allow them to write a title after their names, to enable them to occupy positions in medical schools, or to attract private patients,

—if there be a few such men, they will, of course, object to serving poor patients daily, and will naturally champion the present evil system of short services.

By a continuous service, I mean a service extending over the whole year, except the time when the physician needs to go away for rest—a period of from two to six weeks.

The way of providing a temporary visiting medical officer for this period is a matter of more importance than it is now thought to be, and several plans might be suggested.

The present plan of a colleague being left in charge of the absentee's service, in addition to his own, during the summer months is bad enough with the present arrangement of hospital service, and would be even worse under the plan which I suggest: a physician cannot reasonably be expected to do double duty in a thorough manner in New York during the heated term, and in the re-organized hospital each physician or surgeon would be a quasi-specialist, not actively interested in his colleague's branches.

The plan of having assistant visiting physicians and surgeons I can condemn, understandingly, having been an assistant visiting physician myself for a few months. The real use of such assistants is to save the physician a few hours' time occasionally, by making the visit for him. He, the assistant physician, knows little about the cases in the service, he has no standing in the estimation of the patients and of the internes, and his visits are simply ceremonial. Except in hospitals having out-door departments which can be wholly given over to assistant visiting physicians and surgeons, I am quite positive that making such appointments creates a needless complication, and opens a door to neglect of duty by the visiting medical officers.

The best plan would, it seems to me, be that the physician who intends absenting himself should nominate an acting visiting physician to the medical board and board of managers. This would probably ensure the selection of a competent man, of one interested in the same branches of medicine as the attending physician making the nomination. Besides, such acting visiting

physicians or surgeons would naturally, in the course of time, become desirable candidates for filling vacancies.

It is almost needless to add that with a continuous service in one general hospital there would be no desire to hold a similar position in another hospital. Still, there might be no objection to a physician holding appointments in one special and in one general hospital.

The question of a salary or honorarium for visiting physicians and surgeons is an important one. In Europe—on the Continent at least—hospital physicians are paid a modest sum yearly. This establishes more firmly the contract between the managers of the hospital and the medical staff; and in some cases would aid in securing the services of good men. An objection to a salary is, that it would tend to place the physician in the position of an employé, and destroy the satisfaction of doing some charitable work.

4. The re-arrangement of hospitals into smaller services, with their visiting physicians and surgeons on duty all the year round, would necessitate a corresponding change in the resident staff or internes. This change would consist in the creation of a separate resident staff for each service of the hospital. To do this a much larger number of young men would have to be appointed each year, and increased accommodations for their residence and board would have to be provided for, entailing a considerable additional expense on the hospital. Supposing that, as at present, each staff should consist of three members, a resident physician (or surgeon) and two assistants, the following table would represent the *personnel* of our general hospitals. A column is added to show the number of internes if only the house or resident physician (or surgeon) and one assistant were appointed, or allowed to live in the hospital :

To counterbalance the greater cost of so considerable an increase in the house staff, what advantages would accrue from the change? Great advantages, I believe.

a. Direct advantages : making possible and successful the plan of continuous service of visiting physicians or surgeons; the

thorough cultivation of such service ; the making and writing out of complete observations ; the doing of minute and exact therapeutic applications ; the saving of time which the young men could employ in study ; and, lastly, the health and even lives of promising young men, now endangered by overwork in hospitals, would be made more secure.

Hospital.	Beds.	Visiting Staff.	Resident Staff (3).	Resident Staff (2).
Bellevue	800	13	39	26
Charity	1,000	16	48	32
Presbyterian	100	3	9	6
St. Luke's	209	6	18	12
New York	150	5	15	10
St. Vincent's	250	8	24	16
St. Francis'	200	6	18	12
German	90	4	12	8
Mt. Sinai	160	5	15	10
Roosevelt	180	6	18	12
Total	2,139	72	216	144

b. Indirect advantages : There is often something dangerously narrow and near-sighted in the administration of charities. For example, material relief or medical service is given to applicants without any inquiry as to whether they are proper subjects for charitable assistance, and without attention to the pauperizing tendency of such recklessly-extended assistance. So, it seems to me, is the way in which the governing bodies of general hospitals are apt to look at the institutions under their charge. They fancy that their hospital is created and maintained for the relief of suffering people in their immediate vicinity and nothing more. If that function be well performed, the managers are satisfied, and they seem to have no conception of a hospital as a centre whence a far-reaching beneficence may radiate. To cure three hundred sick annually is a good deed, but is not the completion of the education of young medical men likewise a worthy object ? These men leave the hospital to settle in private practice in the country or city, and what they observed and learned while in hospital is the means of relieving and curing multitudes of sick in the course

of years. Every one in the community is or should be personally interested in the thorough training of young physicians, partly from philanthropic motives, but also from selfish motives, since every one at some time or other comes under a physician's care either at home or while away from the physician of his choice, and there is thus created a personal interest in the general excellence of the medical profession. Now, if the general hospitals of New York could employ a much larger number of young men as internes, and each year send out ten times as many hospital graduates, how immense would be the advantages conferred upon different sections of the country. The function of the hospital to cure and relieve the sick would thus be extended to such proportions that its local good work, that of curing say three hundred sick people annually, would be dwarfed by the indirect good resulting from the practical education and training which the institution had afforded to a succession of internes.

In the table on p. 299 I have inserted a last column, giving the number of internes who would have to reside in the hospitals, but even this might be reduced a good deal, as in many services both assistants might sleep out of the hospital.

There would be, of course, many difficulties in the way of reorganizing hospitals in the direction which experience in Europe has shown to be practicable and eminently useful, and which I have endeavored to sketch in this paper. Encouraged by the words of encouragement which the preceding critical article (see *ARCHIVES* for April, p. 177) brought me from eminent physicians, I venture to throw out the present sketch of an improved hospital service in New York. The following is a summary of the principles upon which the changes ought to be made :

1. The creation of small services, each consisting of from 25 to 60 beds, according to the size of the hospital.
2. Each service to be under the continuous care of one visiting physician or surgeon for a period to be fixed by regulation, say ten or twenty years.
3. The selection of the visiting physicians and surgeons upon the basis of personal and professional merit, and also upon their

special qualifications for taking charge of services intended to receive certain classes of diseases. This would establish the hospital work upon the same basis as the higher class of private practice, viz., upon the principle of the cultivation of specialties, or of departments of medicine.

4. The creation of a much larger staff of internes, first for the purpose of securing better attendance upon the patients of the hospital, and also for the (no less meritorious) object of educating a greater number of physicians previous to their settling in private practice.

E. C. SEGUIN.

NEW BOOKS AND INSTRUMENTS.

Fever : A Study in Morbid and Normal Physiology.

W. C. WOOD, A.M., M.D., Prof. of Diseases of Nervous System in University of Pennsylvania; Member National Academy of Sciences, etc. [Smithsonian Contributions to Knowledge.] Philadelphia : J. B. Lippincott & Co., 1881, pp. 258.

A mere quotation of the principal conclusions arrived at as the result of the 121 experiments recorded and analyzed in this essay, —would quite fail to do justice to the extensive labors involved in their performance. It would also fail to assign a proper place to the researches of the author among those, more or less analogous, of Liebermeister, Leyden, Senator, and others.

The central problem of fever is the relation of heat production and heat retention to it and to each other. It may not be amiss to compare the conclusions arrived at by different authorities in regard to this problem. Thus, "fever is a symptom complex, depending on an alteration in the regulation of heat, by means of which, heat production is increased above the norm, and heat dissipation so disposed (*angeordnet*), that abnormally high bodily temperature results."—Liebermeister.*

"We must *reject* the hypothesis that fever originates in disorder of the nervous centres; that by means of the influence of the nervous system on the systemic functions, the liberation of heat at the surface of the body is controlled or restrained, so that 'by retention' the temperature rises. * * * We are at liberty to adopt the other alternative, that fever originates in the living tissues, that it is from first to last a disorder of protoplasm, and that all the systemic disturbances are secondary. * * * But if we attempt to do this now, we shall at once find ourselves in face of an unsolved physiological problem—that of the normal

* "Pathologie und Therapie des Fiebers," 1875, p. 359.

relation between temperature and thermogenesis."—Burdon-Sanderson.*

"In fever, the elimination of heat is increased, both with a constant, a rising, or a falling bodily temperature. It follows therefore, without doubt, that heat production is also increased. At the height of the fever, the elimination of heat reaches from one and a half to about double the normal amount. It is at its maximum in the critical stage, with rapid fall of temperature, there being from $2\frac{1}{2}$ to 3 times the normal. The defervescence is accompanied by marked sweat secretion and water evaporation; while during a rising fever no evaporation can be discovered. In the epicritical stage, the elimination of heat falls below normal."—Leyden.†

"The excess of bodily temperature during fever cannot be explained by the alterations of tissue metamorphosis, as indicated by its final products, urea and carbonic acid. This excess has different causes, among which *with certainty* may be indicated: 1st. The consumption of tension forces (*Spannkräfte*) stored in the healthy organism for the performance of functions. 2d. The accumulation of heat in the body, during the pyrogenetic period preceding the period of fever. Besides these, may perhaps be reckoned as sources for increased heat production, the greater destruction of albumen to urea, and also processes related in some unknown way to the formation of water. (Increased oxidation of hydrogen according to Auerbach.) The higher temperature in fever is determined by a disproportion between the abnormally increased production and the elimination, which is not increased to the same degree."—Senator.‡

"Fever is a nutritive disturbance in which there is an elevation of the bodily temperature, and also an increase of the production of heat by an increase of the chemical movements in the accumulated material of the body; this increase being sometimes sufficient, sometimes insufficient to compensate for the loss of that heat which is derived directly from the destruction of the surplus food in the body, very little or no food being taken in severe fever. The rise of temperature in fever is, therefore, not dependent altogether upon increased heat production, as in fever there certainly is sometimes less production of heat in the organism than there is at other times when the bodily temperature remains

* Report of the Medical Officer of the Privy Council, 1875.

† Untersuch. über den Fieber, *Deutsches Archiv*, Bd. v., p. 305.

‡ Untersuchungen über den Fieberhaften Process. Berlin, 1873.

normal; also excessive heat production may occur even at the expense of the accumulated materials of the organism without elevation of the bodily temperature."—Wood.*

In this last definition, the only one which the purpose of this review permits us to discuss, it is noticeable that the question of "heat retention," as a factor in fever, is evaded. It may be indirectly referred to in the statement that "the rise of temperature in fever is not *altogether* dependent upon increased heat production:" but neither in the paragraph, nor in the rest of the author's summary, is his opinion on this controverted question formally stated.† In an earlier formularized statement, however (p. 240), the influence of heat retention is much more distinctly recognized.

"The degree of bodily temperature in fever depends, in a greater or less measure, upon a disturbance in the natural play between the functions of heat production and heat dissipation, and is not an accurate measure of the intensity of the increased chemical movements of the tissues."

The author shows, incidentally, in arguing from experiment 114, where the production of animal heat in a pyæmic dog was found to be excessive, 297.7511 calorics hourly as compared with 84.2426 calorics of feeding day, and 60.156 of hunger (apyretic) day, while the bodily temperature remained more than little above normal, that this disturbance between heat production and heat dissipation may assume a different form, and "an excessive heat dissipation may entirely mask an excessive heat production." Thus "the highest development of the fever process may occur when the temperature is lowest" (p. 236).

This is a most important proposition. The physiological experiments contribute largely to explain the clinical phenomena of certain fevers, especially scarlatina, with its low temperatures in malignant cases, as compared with remarkably high temperatures of other cases relatively benign. It is a proposition constantly to be borne in mind in the midst of the present almost overwhelming tendency to measure the severity of all febrile disorders,‡ almost exclusively, by the degree of bodily heat rendered perceptible to the thermometer.§

* *Loc. cit.*, p. 254.

† Traube, it will be remembered, explained all the symptoms of fever by a tetanus of the small arteries, and, in consequence, a diminished elimination of heat.

‡ Except, perhaps, diphtheria.

§ Senator, having accepted the alterations of temperature in fever as a very sure and convenient gauge for the height of the fever, and ordinarily for its

In this connection is to be remembered that Leyden's calorimetric observations on human beings showed an increased dissipation of heat in all stages of fever except the initial stage. There is an absolute increase, even when, relatively to the heat production, it is insufficient. These observations are principally utilized by the author as affording proof of an increased heat production, and as such they are discussed theoretically by Senator, and after him by Burdon-Sanderson, and experimentally by Dr. Wood. We shall, therefore, refer to them again.

To return for a moment to the central conclusion of the author's summary : We feel inclined to criticize the second clause which states that "the increase (of chemical movements in the accumulated material of the body) is sometimes sufficient, * * * to compensate for the *loss* of that heat which is derived directly from the destruction of the surplus food in the body." As a mere verbal correction we would suggest that the substitution of the words "should be" for "is," would make the meaning plainer. But a more important defect in the definition is that it has the air of implying that the increased chemical movements in fever indicate an effort of the organism to produce its requisite heat, when deprived of the ordinary resource of food combination ; whereas, it is very evident that the exaggerated destruction of store albumen, characteristic of fever, precedes the establishment of fever diet, as it is known, by the increase of urea (Ringer, Senator), to precede the appearance of fever. In the next sentence, Dr. Wood closely follows Burdon-Sanderson in saying that "in fever there is less production of heat in the organism than there is at other times when the bodily temperature remains normal," *e. g.*, when a large amount of food is consumed, but when the excess of heat, thence generated, is dissipated by means of the heat-regulating mechanism of the body in normal operation. This proposition is, we might almost say, necessarily true, but to judge of the effect on the organism of a given production of heat, the comparison must be made between the different amounts produced with the same resources. The significance of a given heat production must be entirely different when effected at the expense of food recently added to the organism, or of the stores and forces of the organism itself.

severity, proceeds to point out that "a body whose store of albumen and whose tissues are constantly being destroyed without repair, must, sooner or later, cease to live, whether it have a normal temperature or not" (*l. c.*, p. 177). And "this destruction cannot be attributable in the main to increased temperature, since this can exist, as in tetanus, without any such result following" (*l. c.*, p. 176).

It remains none the less important to call attention to the fact that, without lesion of the mechanism for dissipating heat, increased heat production cannot produce fever. Dr. Wood has performed a large number of experiments for testing the production and dissipation of heat in dogs subjected to a variety of conditions.

There are two ways of measuring the production of heat : 1st, by calculating the quantity and heat value of food consumed. But in fever experiments, it is desirable to eliminate this source of heat production, and the calorimetric measurements have been made, therefore, on fasting dogs. Such measurements constitute the second method for estimating heat production. If the temperature of the body remain constant (observes Burdon-Sanderson) the quantity of heat discharged is *identical* with that produced, and the quantity of heat discharged is estimated by measuring the temperature of water to which the animal body has communicated its heat. To understand the process, however, it is important to remember that it is not the temperature itself, but the kilogramme heat units communicated to the water, which are estimated. This quantity is obtained by multiplying the degrees of temperature to which the water may have risen, by its weight in kilogrammes, since a heat unit equals the quantity of heat required to raise one kilogramme of water one degree.

"Calorimetric measurements," observes Liebermeister, "constitute one of the most difficult tasks of physical investigation. The difficulties are somewhat palliated in physiological calorimetry by the large mass of heat units to be operated on."

As is known, Liebermeister employed a cold bath as a calorimeter for experiments on human beings ; measuring the temperature of the water before and after the body had been immersed for a certain time in it. Many criticisms have been addressed to this method, of which the most obvious is that, as Liebermeister himself admits, the contact of cold water with the animal body greatly increases the heat production of the latter.

After the experiments of Hirn, the nearest approach to precise calorimetric observations on the human body has been made by Leyden. These were partial, that is, one leg only of the fever patient was enclosed in the apparatus. The elimination of heat from the entire limb, was first calculated for each square inch of its surface, then, from this, calculated for the entire body.

Wood's experiments, like those of Senator, were made upon dogs. The apparatus used by the American physiologist is essentially the same as that devised by his German predecessor. But his experiments have been much more numerous (121 as compared with 31 by Senator), and some new problems and calculations introduced into the investigation. On the other hand, the questions concerning tissue metamorphosis in fever, discussed at much length by Senator, are scarcely alluded to by Wood. In only a few of the latter's experiments is the elimination of carbonic acid measured; in none, the elimination of urea.

Some details of the method of experimentation will not be out of place, as they are essential both to the understanding of the problem, and to the appreciation of the amount of labor involved in the research.

The bath of Liebermeister is replaced by a metallic box with double walls. The animal experimented upon is placed in the box; the water to which his body is to communicate heat, is contained between the walls, and thus does not come into immediate contact with the body,—one great defect of Liebermeister's bath. The metallic box is set in a larger wooden box, and the space between the two filled in with some non-conducting substance. Thus, the apparatus remains protected from the influence of the external air. Air for the respiration of the animal is drawn through the inner chamber in which he is placed, by means of external openings, one at each extremity of the box. That for ingress of air communicates freely with the atmosphere of the room; that for egress is connected by a tube with an aspirator, which draws a current of air first through the box, then through a gas meter and around a thermometer.

The heat given off by the animal is conveyed to the water of the calorimeter, to the iron of which it is composed, and to the current of air passing through the box. The quantity of heat (heat units) thus received is ascertained by multiplying together the number of degrees expressing the rise of temperature, the weight, and the specific heat* of the water, the iron, and the air respectively. To this amount must be added the amount of heat involved in the evaporation or condensation of moisture in the air of the box. For this purpose, a sample of the air leaving the box must be examined for moisture, by being passed through

* The specific heat of a substance, it will be remembered, is the *relation* between the amount of heat required to raise the temperature of a substance one degree, and that required to similarly raise the temperature of an equal weight of water.

chloride of calcium tubes, thus introducing a new complication in the apparatus.

Now, as to those unfamiliar with this class of experiments, it is important to emphasize that the result obtained from these different measurements is not yet the *production* of heat during a given time, but only the amount of heat given off or "dissipated" in that time. There is no means of ascertaining heat production by direct experiment; it is estimated by means of a calculation, whose elements are: 1st. The total amount of heat dissipated, as measured by the heat units given to the calorimeter. 2d. An analysis of this heat quantity into two parts of different origin: one, the heat reserve previously stored up in the body; the other, the amount produced in the body during the experiment, *i. e.*, coincidently with the heat dissipation. For this analysis is required a 3d element of calculation, namely, the rise or fall of the temperature of the animal body during the time of the experiment. The rectal temperature is taken before and after the experiment; the difference, of course, gives the rise or fall of heat during the period of observation.

Liebermeister, in a chapter on "The Human Body Considered as a Calorimeter," has pointed out that, if external dissipation of heat were entirely prevented, all the heat produced in an animal organism would go toward raising the temperature of its body. The rise of temperature in degrees, multiplied into the weight and specific heat of the body, would then give the number of heat units produced; the receiving and producing apparatus becoming then, as it were, identical. This fact has been utilized by Wood, although in a different manner from that suggested by Liebermeister, who proposed to directly ascertain heat production by cutting off heat dissipation, the subject of the experiment being placed in a warm bath. Wood, on the contrary, *calculates* the amount of "heat reserve" (*i. e.*, the amount of heat units stored up in the body and employed in maintaining the temperature of the body at any given time) from elements indicated, namely, the rise or fall of temperature, the weight of the body, and its specific heat.

If the temperature has fallen, it is evident that a portion of the heat dissipated has come from the heat reserve. The loss of the reserve, calculated as above described, is therefore subtracted from the total dissipation. The remainder constitutes the amount of heat units which must have been *produced* in order to cover the

amount dissipated. If, on the contrary, the temperature rise during the experiment, it is evident that enough heat has been produced to cover the heat dissipation, and also to add to the reserve. The numbers indicating the two amounts are therefore added together; the sum represents the total heat production.

It seems to us that this calculation is really a considerable addition to the researches of Senator on the problems of heat production and heat retention. Although the conclusions enunciated by Senator, as the result partly of his own experiments and partly of those of Leyden on human beings, sound very positive in the summary, and as quoted by Burdon-Sanderson, closer examination shows that their author feels a good deal of uncertainty about them.

"The alterations of heat dissipation," he observes, * * "may be explained without any alteration of heat production; and the fact that the bodily temperature is maintained above the norm is quite compatible with *no* increased heat production, since calorimetric experiments have demonstrated, beyond a doubt, that pyæmic fever begins with a retention of heat. * * * In case heat production is on the whole increased during the fever process, it is certain that in the dog, at least, this increase cannot be great. * * A moderate increase corresponds with alterations of tissue metamorphosis (shown to be quite insufficient to explain the entire rise of temperature). * * * Observations, as on the 2d fever day of the 1st series, on the 3d fever day of the 3d and 7th, when more heat was dissipated than in the corresponding apyretic day—and, nevertheless, the body, at the end of the experiment, became warmer than at the beginning,—justify the opinion that *here* an abnormal amount of heat has been produced. On the other hand, observations in the 2d, 6th, and 7th series, where the animal dissipated abnormally little heat, and yet the temperature of his body fell, should lead to an entirely different conclusion." (Senator, *l. c.*, pp. 85 and 83.)

In Wood's six experiments the heat production and heat dissipation remained constantly parallel, rising and falling together. This appears from the summary of a single experiment, which, in this particular, is a sample of all. The calculation is in heat units.

The bodily temperature, however, is in no fixed proportion to the observed heat dissipation, nor, consequently, to the calculated heat production. In some cases it remained unchanged. Then the production and dissipation of heat were necessarily

equal. Thus (p. 191), "12th period. No change in bodily temperature. Hourly dissipation of heat=hourly production of heat 126.7412." In other cases it fell, though the dissipation of heat was less. Thus, "11th period. Fall of temperature in 1 hour 0.055° , hence heat taken from reserve=1.60875, while hourly dissipation of heat=117.946, and (calculated) hourly production =116.337." Sometimes it rose, while the dissipation of heat remained the same or increased, as, "7th period. Rise of temperature in 1 hour= 0.109° . Heat units added to reserve=3.04. Hourly dissipation of heat=117.0084. Hourly production, however (calculated from addition of two),=120.1966."

Day	Time in calorimeter.	Avg. hourly heat dissip.	Avg. hourly heat prod.	Avg. rectal temperature.
Food day . .	17 hours	132.7014	139.4733	104.07°
Hunger day . .	18 hours	129.498	128.0702	104.78°
1st fever day . .	15 hours	131.5025	130.1177	104.89°
2d fever day . .	20 hours	134.243	133.256	105.39°

It is evident that the basis of calculation for these different elements is essentially the same as in Senator's experiments. In them increased heat production was positively inferred only when the bodily temperature rose at the same time that more heat units were found to have been given off to the calorimeter. Rise of bodily temperature coinciding with a diminished radiation of heat to the calorimeter may always be supposed to depend upon heat retention. It is the fact that, in all stages of fever but the initial one, heat dissipation has been proved to be increased while the bodily temperature rises, which constitutes the principal basis for the theory of increased heat production as an essential part of the febrile process.

Senator himself observes that his own experiments are inadequate for the full solution of the problem, because they do not extend over a sufficient length of time. There is reason to believe that both heat production and heat retention are constantly varying during different periods of the fever, and even of a single diurnal cycle; and that the aspect of the morbid process, at any given time, is the result of a varying combination between the increase or the diminution of these two factors.* Wood has extended his observations over fifteen hours instead of three.

* To which Senator adds the variations, observed or inferred, in the tissue metamorphosis and in other processes, upon which heat production must be supposed to depend.

In each experiment the animal was examined for twelve different periods, so distributed throughout different portions of the four days that every part of the diurnal cycle could be tested. This extension constitutes an important advance upon the earlier experiments. But it is interesting to notice that the conclusions arrived at do not in any way modify the earlier ones, but are, as the author remarks, "in close accord with those of Senator."

"In the pyæmic fever of dogs, the heat production is usually in excess of the heat production of fasting days, but less than that which can be produced by high feeding; usually the production of animal heat rises in the febrile state with the temperature and with the stage of the fever, but sometimes the heat production becomes very excessive, although the temperature of the body remains near the normal limit.* In rabbits with pyæmic fever heat production seems to be even greater than it is in health when food is taken" (p. 236).

To the clinician not well acquainted with these recent and most delicate researches in physiology, it may be quite a surprise to learn that excessive temperature and excessive heat production are not identical. It is a most capital fact, however, to bear in mind, if only for its practical bearing on the limits of antipyretic therapeutics.

Before studying the thermic phenomena of fever by means of these experiments on pyæmic dogs, Dr. Wood records a number of experiments performed to investigate the normal mechanism of heat production and heat retention. These are preceded by twenty experiments, showing the effect upon the animal organism of a sudden elevation of its temperature by means of heat communicated from without either to the entire body or to the head. These latter experiments suffice to demonstrate that "external heat applied to the body of normal animals, including man, so as to elevate the internal temperature, produces derangements of the functions of innervation, of respiration, of circulation, etc., etc., precisely similar to those seen in natural fever; the intensity of the disturbance being directly proportionate to the rise in temperature. * * * The withdrawal of the excess of heat in acute fever is followed by a relief of the nervous and circulatory disturbances" (p. 12). (The latter proposition is demonstrated by two interesting clinical experiments, in addition to those performed on artificially heated animals.)

* The excessive heat production being masked by an excessive heat dissipation.

"It would appear to follow as a direct corollary to these propositions that excessive temperature is the essential symptom of fever."

Now it seems to us, that in the definition of fever given by the author himself at the conclusion of his essay, and which we have already quoted, the absoluteness of this proposition is very greatly modified. In his summary, Dr. Wood tells us "fever is a *nutritive disturbance* in which there is an elevation of bodily temperature," etc. Here, very properly we think, the essential phenomenon of fever is identified with the "nutritive disturbance." To what extent this may be accompanied by an elevation of bodily temperature, depends, as is abundantly shown by the author in confirmation of earlier experiments, upon quite a variety of circumstances.

The fact that an artificially induced rise of temperature *may* induce derangements of innervation, etc., similar to those occurring in natural fever, by no means proves that in a given case of such fever the other morbid symptoms *are* caused by the bodily heat. For nerve centres, having only limited modes of reaction, may react in the same manner to the direct influence of a poison circulating in the blood. It is only thermic fever (sunstroke) that is exactly imitated by experiments of overheating. The great majority of clinical cases of fever are associated with the introduction into the organism of a specific poison; and are, therefore, only paralleled experimentally, by the intravenous injections of pyrogenic substances. Dr. Wood recognizes (pp. 12, 13) that in natural fevers "there may be a poison which may modify or entirely suppress the symptoms which the increased temperature would normally produce." But he does not recognize the converse proposition, that this same poison may be the cause of precisely such symptoms as *may* be produced by heat alone. Thus, ataxic delirium (the equivalent in adult human beings for convulsions in the lower animals) may certainly occur in typhoid fever, with only a moderate elevation of temperature. Similarly, the convulsions which sometimes mark the onset of severe scarlatina, may entirely precede any excessive rise of temperature.

Dr. Wood's researches, like those which have preceded them, also suggest an inquiry into the morbid effects of increased heat production when masked by increased heat dissipation, as distinguished from the effects of rise of bodily temperature.

Not only in the nervous and vascular symptoms of fever, but

in the lesions produced during its course, is it necessary to keep in mind the possibility of a double causation,—on the one hand, the high temperature; on the other hand, the direct action upon the tissues of the original poison. In the practical matter of antipyretic treatment, hardly any two febrile diseases can be compared with each other. Thus, in cerebral rheumatism, the dangerous symptoms do seem to be clearly traceable to sudden heat retention, as indicated and caused by cessation of the profuse sweats that had hitherto kept the bodily temperature within moderate limits, notwithstanding the increase of heat production. Hence, the immediate abstraction of heat is pressingly indicated, and is capable of effecting a rapid and permanent cure. But in prolonged infectious fevers, where the conditions are much more complex, the advantages of the immediate abstraction of heat, by means of cold-water applications, must be compared with possible disadvantages resulting from the increased heat production, which, as Liebermeister has shown, is the *first* effect of these applications.

In studying the mechanism by which the normal animal organism controls the production and dissipation of heat, Dr. Wood discusses experimentally the following problems :

Effects on heat production and heat dissipation of section of the spinal cord. (Exps. 21-24, on rise of temperature. Exps. 25-39, with calorimeter.)

The situation of the chief vaso-motor centre, and the effect on thermogenesis of high section of the medulla. (Exps. 40-53.)

The effect on heat dissipation of paralysis of the vaso-motor centre. (Exps. 54-59.)

The effect on the thermic functions of wounds of the cerebellum. (Exps. 60-62.)

The possible existence above the pons of a heat-regulating centre. (Exps. 72-74.)

The connection between the cerebral cortex and the thermic functions. (Exps. 76-97.)

Possible existence of a muscular vaso-motor centre. (Exps. 98-109.)

Calorimetrical observations, taken, as has been described, before and after section of the cord, showed that this operation was followed by a marked increase of heat dissipation, and coincident lessening of heat production. The author attributes both results to "vaso-motor paralysis, which acts directly (by slackening circulation), and also indirectly, by causing an excessive loss of heat,

and such a lowering of the internal temperature as to check the chemical reactions in the body" (p. 45). The total effect is the marked fall of bodily temperature, which has always been observed after section of the cord.

When section of the medulla was made, through the vaso-motor centre, *i. e.*, just above the point of the calamus, the effect on thermogenesis was the same as that of section of the cord. When, however, the section was made above the vaso-motor centre, *i. e.*, at the junction of the medulla and the pons, not only heat dissipation but heat production was found to increase, the former not keeping pace with the latter, so that the bodily temperature rose. This confirmed the experiments with a similar section, which have been made by Tscheschichin, and also by Bruck and Günter (quoted by author), and which have led to the theory of an "inhibitory heat centre," situated in the brain, in or above the pons. Wood's experiments tend to confirm this theory, and to overthrow that advanced by Heidenhain, namely, that the rise of temperature is due to irritation of the vaso-motor centres. Heat production was found to increase progressively from the time of the section until 23 hours after it; while, if due to irritation, the heat production should have reached its maximum at once, and then progressively declined. The conclusion is that this heat production was "paralytic, and due to the removal of some active force" (p. 82). It is pointed out that "this theory involves the exercise of a controlling influence of the nervous system upon the nutrition of the body." This remark implies that an excess of heat production always can and must be explained by an increase of the chemical processes, oxidations of the organism. But, as we have seen, either in natural or in artificial fever, induced by other means than medullary sections, the increased oxidations, which can be demonstrated and measured by the elimination of carbonic acid and urea, are insufficient to explain the rise of temperature. We have already quoted the suggestion of Senator, that a portion of fever heat results from the diminution or arrest of work in various portions of the organism, and the coincident liberation of the mechanical heat equivalent of such work. It seems to us that such a mechanical source of heat may be invoked to explain the high temperatures observed after section of the medulla below the pons, with consequent generalized motor muscular paralysis.

The convergence of motor tracts in the pons has been admitted to explain the occurrence of generalized convulsions, when this

part of the brain is irritated, without the hypothesis of a "special convulsive centre." When a section is made between the medulla and the pons, cerebral innervation of voluntary muscles is completely interrupted, and, to that extent, the liberation of motor force prevented. The formation and explosive decomposition of contractile substances in the muscles, however, continue, but the chemical force which is normally converted into both heat and motor force, must now be converted entirely into heat. Hence, we submit a possible cause of rise of temperature, which should increase progressively for some time after the section.

By numerous calorimetrical observations, taken before and after lesions of different portions of the encephalon, it was shown that wounds of the cerebellum produced no perceptible effect upon thermic functions, but that destruction of the first cerebral convolution posterior to the sulcus cruciatus (Hitzig's region) is followed by an increase in heat production; while irritation of the same nervous tract (salt being placed on the exposed brain) is followed by a decided decrease of heat production. "It seems probable, however (observes our author), that these effects are temporary; that this first convolution does not contain calorific centres, but is, in some way, connected with these centres."

It is asserted that these centres are probably situated in the pons; but no experiments are adduced in support of this assertion.

While some influence over heat production was found to be exercised by the cortical centres, Dr. Wood failed to discover any evidence of a vaso-motor action in these same centres. The test of such influence is always galvanization of the sciatic nerve after destruction of the region investigated. If that region contained a vaso-motor centre, irritation of a sensitive nerve after destruction of the region should fail to cause a rise in blood-pressure, since such rise implies the integrity of the vaso-motor centre. The application of this test showed that arterial pressure was quite unaffected by either destruction or irritation of the cortical centres, and this whether the medullary centre were intact, or whether, in order to eliminate its influence, it had been separated by section of the medulla. "Consequently the rise of heat production following section of the medulla is due to an influence not exerted upon the circulation, but directly upon the heat-making function" (p. 157).

This conclusion would not be incompatible with our own interpretation of Tscheschichin's experiment.

In the final chapter of the essay, the author discusses the general theory of fever ; its hæmic or neurotic origin ; the possibility of purely "irritative fever."

In concluding upon these two last questions, Dr. Wood places himself in direct opposition to Burdon-Sanderson, in maintaining that "fever occurring in cases of blood poisoning is often, and probably always, the result of a direct or indirect action of the poison upon the central nervous system, and, hence, is a neurosis. Irritative fever, if it exist, is produced by an action upon the nervous system" (p. 248).

The relations of the vaso-motor system to the febrile state are investigated by comparing the effects of sections of the cord and consequent vaso-motor paralysis in the normal and fevered animals. It was found that, in that latter, the increase of heat dissipation and diminution of heat production, caused by the lesion, were both much exaggerated, over what was observed in a healthy animal. Dr. Wood infers that "the general vaso-motor nerves restrain heat dissipation more completely in fever than in health, and consequently the effect of their sudden palsy is more marked."

This observation is in accord with Senator's on the extremely irritable condition of the vaso-motor nerves in fever, which determines irregular periods of heat retention.

The condition of the so-called inhibitory heat centre in fever was examined in two sets of experiments. In the first, a few hours after the development of fever by subcutaneous injection of pus, galvanism was applied to the femoral nerve, with the effect of causing just such a fall of temperature as has been observed by Heidenhain and others in normal animals. But Heidenhain, using feeble currents, failed to produce this fall of temperature in fever ; hence inferred the paralysis of the inhibitory heat centre.

In a second research the comparative experiments were made on normal and on fever days ; and the irritation was applied to the ear. In this case these irritations were found to have less effect in depressing the temperature than in the normal animal.

We do not think much importance should be attached to these experiments until the mechanism of the thermic functions of the pons be more firmly established. In his summary, Dr. Wood observes : "The only nerve centre proven to exist, capable of influencing heat production without affecting the general circulation, is situated in the pons or above it, and whilst it may be a

muscular vaso-motor centre, it is more probably an 'inhibitory heat centre.' "

"In the pons or above it" leaves a very wide margin, and we have seen that the experiments upon which the above proposition rests are susceptible of a different interpretation.

Still less is there at present sufficient ground for the ninth proposition of the summary: "The so-called inhibitory heat nervous system is not paralyzed in fever, but is less capable than in health of answering promptly and powerfully to suitable stimuli; in other words, is in a condition of paresis or partial palsy."

The eleventh and final proposition is that "in all cases of serious fever there is a definite poison circulating in the blood, the poison sometimes having been formed in the system, sometimes having entered from without."

It is this depressing poison which acts on the heat-regulating nervous apparatus; the inhibitory centre is depressed and benumbed; and, hence, tissue change goes on unrestrained; more heat is produced; there is abnormal destruction of tissue. Vaso-motor and other (?) heat-dissipation centres are also benumbed, and do not provide for dissipation of animal heat till it becomes excessive.

This last proposition is rather in contradiction with the author's own experiments, showing that vaso-motor palsy is followed by an excessive dissipation of heat.

We have made this long abstract of Dr. Wood's essay because it was needed in order to give any just idea of the range of physiological problems discussed, and of the immense experimental labor which has been devoted to the discussion. From what has been said, it is clear that the value of these important researches consists rather in the confirmation they afford to others already performed, or to theories already distinctly formulated, than to any marked originality in either method or conclusion. The memoir is in no sense "*bahnbrechende*," but its laborious and complex investigations will find on that account no difficulty of acceptance as a solid contribution to the solution of some of the most delicate and abstruse problems in modern physiology. We are not aware that, in this science, any work of similar magnitude has been performed on this side of the Atlantic. It probably could not have been completed without the assistance of the several brilliant collaborators to whom Dr. Wood, in his preface, offers a graceful tribute of thanks. While confirming the principal conclusions of Liebermeister, Leyden, and Senator, its experimental researches

considerably exceed those hitherto performed in multiplicity, range, and variety; in care and accuracy it yields to none; and, in some details of calculation, has made a real advance in method. The more familiar the reader may have become with these abstruse researches, the more inclined he will feel to accord high praise to this latest publication of the Smithsonian Institute.

[M. P. J.]

Syphilis and Marriage. Lectures delivered at the St. Louis Hospital, Paris. By ALFRED FOURNIER, Professor, etc. Translated by P. ALBERT MORROW, M.D., etc. New York: D. Appleton & Co., 1881, pp. 251.

If we except the work of Langlebert (*La syphilis dans ses relations avec le mariage*, Paris, 1873), the series of lectures included under the above title affords the only book devoted to an exhaustive presentation of the subject of syphilis in its relations to marriage. Its importance has induced Prof. Fournier to communicate the results of his own experience, most fruitful in this, as it has been in all fields relating to syphilis. While the consideration of this subject necessarily includes its moral as well as its medical aspects, the author prefaces his work with the caution that the physician, in determining the many embarrassing questions submitted for his decision, should be guided solely by scientific motives. He has no right to be influenced by, has no right to consider even, the arguments of the syphilitic man contemplating marriage, persuading him to sacrifice his judgment to sentiment or questions of expediency.

And yet it would never do to condemn to celibacy every man who has had syphilis, for there can be no doubt that syphilis affords "only a *temporary* interdiction to marriage" in many cases, and that one may often permit such marriages with the certainty that neither wife nor child will be exposed to contaminating influences. Dangers due to syphilis in marriage are considered under three heads, viz., those to the wife, those to the children, and those to the common interests of the family. The wife is liable to infection: 1. By the transmission of a contagious lesion occurring to the husband after marriage. 2. By contamination, after conception, from the embryo made syphilitic by the sperm of the father. The first method of infection is admitted universally, but the author has not given at sufficient length the arguments of those who deny the possibility of the second mode. By Kassowitz and others, it is positively asserted that the embryo can only become syphilitic through the germ or sperm-cell, never

by intra-uterine infection subsequent to conception. They also claim, conversely, that unless the mother becomes syphilitic by direct contagion, the barriers between the maternal and foetal circulations will prevent her infection from the foetus, syphilitic from its father. It is most probable, indeed, that these writers are incorrect, but the weight of their authority should certainly entitle their opinions to greater consideration than is here shown them.

A syphilitic man may become dangerous to his offspring. This, however, does not of necessity follow, since many men have had syphilis before marriage, and yet never infect their wives nor children. The author declares he has seen men in the full secondary period of syphilis, at the very moment of the fertile coitus, procreate healthy children. If we admit the paternity of such children, if men in this condition have procreated healthy children, one cannot avoid the conclusion that syphilis is not a disease pervading the tissues of the body universally and uniformly, but rather that it must affect them in a manner comparable to that in which the yeast cells are present in the fluids that nourish them; that, in other words, it depends upon a particular virus and not to a simple nutritive influence. The author claims that the inheritance of syphilis from the father alone is more rare than is generally believed.

A common result of the paternal syphilis is non-viability of the foetus. It dies *in utero*, or soon after birth; or, finally, may live with enfeebled constitution, with slight power of resistance, with a tendency to nervous accidents, "notably to convulsions," etc.; children thus born often die suddenly, without apparent cause, after a few days or weeks. The father may also imperil his children in communicating his syphilis to his wife. When both parents are syphilitic, the foetus almost certainly becomes infected. Under such conditions: (1) It may die before birth; (2) it may be born syphilitic; (3) a few cases may escape entirely, but others, escaping active lesions, may be born wretched, stunted, feeble, eventually to fade away into death, or to die suddenly, as already mentioned; or, escaping death, they may live "with poor and debilitated constitution, with a condition of anæmia, persistent, and rebellious to all treatment, with a vital resistance inferior to the normal average." The author also believes that such children are predisposed to affections of the nervous system. They are liable to convulsions, to meningitis. They are often imbecile, idiotic, hydrocephalic. They are disposed to scrofula. However, the

author does not claim a syphilitic origin for scrofula. Syphilis is for him one of the "affluents of scrofula." Syphilis of the mother is pernicious to the fœtus. Under this condition, an astoundingly small proportion of infants survive.

The author then considers the dangers to the interest of the family of a man whose syphilis is not yet extinct. Such a man is at any moment liable to the lesions that may quite incapacitate him for the duties of life, and has no moral right to imperil the interests of others.

From all these considerations it is concluded : (1) That marriage should be forbidden to every man who still presents a syphilis sufficiently active to be dangerous ; (2) that, conversely, it may be permitted to every man in the opposite conditions. When, therefore, may one who has had syphilis, marry ?

Fournier formulates the following requirements : 1. Absence of existing specific accidents. 2. Advanced age of the diathesis. 3. A certain period of absolute immunity consecutive to the last specific manifestation. 4. Non-threatening character of the disease. 5. Sufficient specific treatment. The more recent the disease of the husband, of course, the more dangerous is he to wife and offspring, and while there remains a single specific lesion, the interval of time since infection should never induce the physician to permit his patient to marry. Fournier is disposed to require a long period of immunity from the date of the last manifestations. Marriage should be forbidden until after a *minimum period of three or four years after infection*, devoted to a most careful treatment. He would even prefer a longer interval, only "tolerating" marriage at the period named, and then only after the patient has passed from eighteen months to two years without any syphilitic manifestation. The character of the diathesis should not be menacing ; the lesions should not have evinced a marked and persistent tendency to recur ; they should not have been of a grave or destructive character ; they should not have involved important organs. Where the brain has at any time been the site of specific lesions, the patient should not be permitted even to discuss the subject of marriage. As a final condition, the author requires prolonged and active specific treatment. Fournier is not a believer in the benignity of syphilis. Upon this point his views are, perhaps, a little too extreme. "Syphilis, treated insufficiently or not at all, terminates certainly, constantly, in grave tertiary lesions. at a period more or less advanced."

Specific treatment constitutes the best safeguard against the

personal risks that surround the husband. "It diminishes and suppresses the *hereditary* risks of syphilis." It likewise diminishes and suppresses the chances of contagion in marriage. Both paternal and maternal heredity are amenable to its influence. Three or four years of such treatment are essential, even in the most benign forms of the disease. Those who completely satisfy these conditions may *safely* be permitted to marry; none others.

Part ii of the work concerns syphilis after marriage. Syphilis has been introduced into the family, and the interest centres in the dangers resulting from this situation. When the husband is syphilitic, the wife healthy and not pregnant, three dangers threaten. These are those resulting to the husband, the danger of infecting the wife, and that from heredity, for the child. Here it is important to cause the lesions to disappear as rapidly as possible. Specific remedies may be used more actively than usual. The sources of contagion should be immediately destroyed by cauterization, etc. Contagious accidents of the secondary period should be treated with especial intensity. Contagion is most often conveyed to the wife from secondary accidents, usually benign in appearance. Matters are more complicated when the husband is syphilitic, the wife healthy but pregnant. When the man has become syphilitic subsequent to the conception of his wife, all that is necessary is treatment that will protect the wife. When, however, the husband was syphilitic at the moment of conception, the problem is much more complex. The child may be or may not be healthy; it is impossible to decide. Fournier confesses his embarrassment. Here also arises a question of etiology that has never been definitely decided: Can a healthy woman bear a syphilitic child? Can she carry a syphilitic foetus for nine months without becoming herself contaminated? When an apparently healthy woman is married to a syphilitic man and suffers from a number of abortions, but finally bears a healthy child, *after specific* treatment, is it the condition of the mother or of the child that is thus corrected by the medicine?

Another phase of the subject is where a syphilitic man has infected his wife. For the husband the treatment remains as before. What shall be done with the wife? She does not know the nature of her malady; the peace of the family requires that she should remain ignorant of it, and yet her health, even her life, depends upon the thoroughness of her treatment. Treatment, such as the conditions demand, can be, with difficulty, instituted and carried

out in one who is being constantly persuaded by the guilty author of her trouble, that it is of little consequence and will be of brief duration. Too often the wife is much less thoroughly treated than the husband, and is often condemned to a life of suffering, and a miserable death, while her better-informed spouse escapes. It is not altogether clear that the woman should be kept in such absolute ignorance as Fournier recommends. And yet there are few of us who do not constantly become accomplices in the deception; and, indeed, the knowledge of the truth could not prove an unmixed blessing to the wife. It is evident that if deceit must be practised, the physician should stipulate that the wife's anxiety should not be cunningly and treacherously allayed on account of the husband's fears of detection.

Finally, the husband may be syphilitic, the wife syphilitic and pregnant. Here the efforts of the physician must be directed toward securing the birth of a viable child, through energetic specific treatment of both parents. If this be neglected, disaster will almost inevitably ensue. Treatment may prevent abortion, may even secure the birth of a healthy child. The condition of the mother need be no obstacle to the administration of mercury, judiciously prescribed.

There are still other dangers than those to the family of a syphilitic person. Those who are brought into relations with the infected are imperilled. The child may communicate the disease to its wet-nurse, from whom the contagion may spread still further. It is, therefore, imperatively enjoined upon the physician to see that the infant be nursed by its mother, or, if that be impossible, that it be fed artificially. Under no circumstances should a healthy woman be exposed to the almost certain dangers of infection. It may be objected that there may be conditions where it would be very perilous to the mother to nurse the syphilitic infant she has borne without herself becoming infected, and, conversely, that one should not expose a child that has escaped contamination in the womb of its syphilitic mother, to risks of extra-uterine contagion. This opens a most interesting and important question. However plausible the theory that a syphilitic child may be born of a healthy mother, the fact remains that there has never been recorded an instance where a congenitally syphilitic child has infected its own mother. A healthy nurse, who suckles a syphilitic child of another woman, will run great danger of infection. What does this difference signify? It can only mean that the mother escapes because she is not the healthy

woman she may appear to be ; she has, in some manner, become infected with the syphilitic poison. Otherwise, she could not fail to become syphilitic in many instances. This "law of Colles" is worth any number of observations of so-called healthy women bearing syphilitic children, and so long as it remains unchallenged, we must believe that the mothers do not escape. Moreover, there seems no reason to believe that a woman who has been syphilitic during pregnancy may communicate syphilis to her offspring *extra utero*. Fournier has never seen such a case ; and yet it would seem that such a disaster might very well happen, if the syphilis of the mother were acquired toward the end of her pregnancy.

A mother, then, need never hesitate to nurse her syphilitic offspring, and, according to Fournier, unless on account of general hygienic considerations, need not refrain from nursing her child, should she, herself, be syphilitic.

The work presents all the aspects of the interesting problems relating to syphilis and marriage, and, consequently, cannot fail to be valuable to the profession. The translation is excellent, though, here and there, an unnecessary preservation of the French idiom is to be observed. [I. E. A.]

Lectures upon Diseases of the Rectum, and the Surgery of the Lower Bowel. Delivered at the Bellevue Hospital Medical College, by W. H. VAN BUREN, M. D., LL.D. (Yalen.), Professor of the Principles and Practice of Surgery in the Bellevue Hospital Medical College ; one of the Consulting Surgeons of the New York Hospital, etc., etc. Second edition. New York : D. Appleton & Co., 1881. pp. 403.

So pleasant a task as the perusal of this work of Dr. Van Buren's rarely falls to the lot of the reviewer. In the present era of book-making, and specially in this country, the manner of conveying knowledge is too much ignored, the matter chiefly occupying the writer's mind, to the manifest injury of American medical literature. Dr. Van Buren's book is, therefore—as is all that he has written elsewhere,—a model to be copied by aspiring authors. His style is clear, terse, and elegant, and does not demand a laborious study, sentence by sentence, as if it were a foreign tongue. This may seem an unnecessarily severe slur to cast upon medical writers in general, but there is more than one text-book—and much more of our current medical literature—which is manifestly obnoxious to the charge.

The reviewer too often finds it a difficult task to discover points

to praise, in order that his criticisms may not seem one-sided and unjust. These lectures, however, place him upon the other horn of the dilemma, viz., to find somewhat to criticise severely enough to clear himself of the charge of indiscriminating laudation. Of course, the author upholds some views which conflict with other authorities, but he substantiates them by the most powerful of arguments, viz., a large experience, the results of which are enunciated by one who elsewhere shows that he can appreciate, and accord the due value to, the work and experience of others.

The book is divided into twelve lectures or chapters, and is sufficiently illustrated by twenty-seven wood-cuts, which are chiefly borrowed, but scrupulously acknowledged. The first lecture opens with a most able and exhaustive dissertation upon that most annoying of complaints, so-called pruritis ani. He demonstrates what is so commonly overlooked, that there is, in all probability, *no such disease*, it being merely a symptom, perhaps reflex, from some distant source of irritation. He gives, among other reflex causes, irritation of the prostatic urethra, even where no stricture exists. Then, too, his fertility of resource in treatment is remarkable, demonstrating the fact that he is no mere routinist, but a scientific therapist, applying his general knowledge to special cases. The remainder of this chapter, and the whole of the second, is devoted to a remarkably lucid and valuable consideration of the subject of hemorrhoids. The recapitulation occasionally resorted to deserves special commendation, serving to fix in the reader's mind the essential points to be remembered, divested of all superfluous description. We have never before read so admirable and scientific an account of these affections. Let us specially note that here and elsewhere his pathology is up to date, is laid great stress upon, and is, as it always should be, made the basis for treatment. He does full justice to the views of others by extensive and judicious quotation of authorities, and, where their views conflict with his, evinces a spirit of scientific fairness and courtesy, which we wish could be more commonly followed. He is wedded to no special method of operating, and insists that it is not sufficient to diagnose the case as one of hemorrhoids, but also the stage, variety, and *present pathological condition* of the tumors. He clearly indicates in what cases nitric acid may prove useful, but, by example and quotation, demonstrates it to be a dangerous agent, "except in skilful hands." The new treatment by injection of carbolic acid, instead of being either extravagantly praised or condemned, is, with certain pre-

cautions, declared to be likely to prove successful where the hemorrhoid is "still soft throughout, and situated well above the sphincter." This single quotation sounds the key-note of the whole work, viz., a thorough knowledge of pathology, a trained power of observation, which detects the actual condition in each case, and a thorough mastery of the principles of surgical therapeutics. The operation which Dr. Van Buren prefers is ligation by transfixion, after dissecting the pile from its cutaneous attachment, preceded by a "gentle but forcible stretching of the sphincter." This plan, having been pursued by the author for twenty years without evil result, is entitled to the careful consideration of practical surgeons, notwithstanding the theoretical objections which have been urged against it. It certainly enables the surgeon to operate with more ease, obviates the pinching of the inflamed structures, and by thus avoiding irritation, Dr. Van Buren thinks, prevents retention of urine. Personally, we object to the transfixion as unnecessary, and apt to split one or more of the veins contained in the pile. It is also unnecessary, as we have never failed, after a sufficiently free dissection, in strangulating the pile *without* transfixing it. We also differ from him in thinking that the actual cautery "is nearly on a par with the ligature," as we consider it superior, for the reasons which have been so ably stated by Henry Smith. We would also suggest that, in the absence of the thermo-cautery, the ordinary "button-headed cautery be used," should read, "the hatchet-shaped, or olive-pointed cautery," for the surfaces to be cauterized are, owing to retraction, *concave*, and the vessels are usually situated at the deepest part of the cleft, where it is difficult to effectually apply the button-headed cautery.

He distinctly denies that he has ever seen evil consequences arise, as is the popular belief, from arresting the hemorrhoidal flux. His advice as to palliatives, where consent to more radical measures cannot be obtained, is worthy of the attention of the general practitioner, who usually knows no mean between advising his patient to submit to surgical interference, or prescribing some comparatively useless ointment.

Prolapse of the rectum is treated of most thoroughly in the third lecture, which is an unusually long one. The characteristic radiating folds in the partial form of the disease with the deeper and "uniformly transverse" ones of the complete prolapse are well shown by two contrasting illustrations. The partial form when occurring in the adult, the author thinks, "is almost always

caused by hemorrhoidal tumors." When uncomplicated, it is usually found in children, and when neglected, "results in dragging down of the remaining coats of the bowel, *i. e.*, complete prolapse."

Dr. Van Buren describes three distinct varieties of the complete form, the first of which is the result of a "persistence of the causes which are keeping up" the partial prolapse, continuing to act "when the submucous tissue will no longer yield." In the first variety the "finger passed around the base of the tumor recognizes that its external surface is absolutely continuous with the membrane that lines the orifice of the anus, without the existence of a sulcus." In the second, "the finger can be inserted into a groove alongside of the base of the tumor, so as to recognize a distinct sulcus of more or less depth, at the bottom of which, if not too deep, the lining membrane of the gut can be felt as it is reflected from the base of the protruding tumor." In other words, it is "invaginated."

In the third variety "the finger can be passed through the anus alongside of the protruding tumor, but cannot reach any line of reflection of the mucous membrane of the rectum upon the tumor; the latter, in fact, may not even as yet have protruded externally through the anus, but may be felt only as a sort of polypoid mass occupying the cavity of the rectum." Here the invagination has occurred high up. All these forms of prolapse contain peritoneum, and, as they involve "chronic and generally extensive invagination," must prove eventually, "almost of necessity, fatal." As to their relative frequency, the common belief that the partial is the one more usually met is, in Dr. Van Buren's experience, wrong, as even in children, if the prolapse is large or of long duration, it most commonly "contains all the coats of the rectum." Space forbids a detailed account of the various palliatives suggested. The radical operation advised ("except where irreducible invagination exists") is as follows, and its success is dependent, the author insists, upon clearly recognizing the original causes in operation, *viz.*, a yielding of the submucous cellular coat and a patulous condition of the sphincter.

Dr. Van Buren declares, as the result of his experience, that the actual cautery applied longitudinally in parallel lines or at isolated spots is, *par excellence*, the operation for the cure of "prolapse amenable to local means." With regard to the gravest form, invagination high up, he advises that the usual means of injecting air, water, etc., should be tried, but thinks that they are rarely efficient, and that laparotomy should therefore be relied on

in the event of their failure. He most unqualifiedly recommends this operation in appropriate cases, and thinks that success will be more frequent now than formerly, by the aid of antiseptics, and the experience of peritoneal surgery obtained by ovariectomy.

The fourth lecture treats of polypus and benign tumors. The author points out the exceedingly narrow boundary existing between adenoma and cancer. He makes the observation which, if confirmed by the experience of others, will be a very important fact, that, in general, growths which are attached by a distinct pedicle are benign, while the more sessile they are the worse the prognosis, *whatever the histological appearances may be*. In speaking of the rare affection described by Broca as circumscribed poly-adenomata, he states that he is confident that he has seen decided benefit follow a prolonged milk diet. "In a child, bleeding from the rectum, especially if a reddish-looking protrusion is also spoken of, means polypus, for bleeding piles do not occur in children." Even in adults hemorrhage is the prominent symptom in polypus. To distinguish an indurated, partially pedunculated, internal hemorrhoid from a polypus, the author gives the positive point, that while the latter "is always attached well above the sphincters," "the hemorrhoid tumor is confined to its own locality below." Where the pedicle is small, the author ligates in mass, while, when broader, he prefers removal by the knife and scissors followed by careful ligation of every bleeding point. In mentioning the very rare affection, lipoma of the bowel, which may become pedunculated, he points out that upon section such growths present a reddish-brown color. This may explain their apparent rarity, as a careless observer, misled by their unusual appearance, might readily consider that he had one of the more ordinary forms of polypus to deal with. This whole lecture is replete with useful facts, which we regret that space forbids examining more in detail.

In the fifth lecture the subject of abscess in its various forms is most fully treated of. Three main varieties are recognized, viz.: "marginal," situated immediately at the orifice; ischio-rectal; and those taking their origin in the loose connective tissue surrounding the rectum above the levator ani muscle, in the so-called "superior pelvi-rectal space" of Richet. The term "idopathic" applied to ischio-rectal abscesses, the author thinks, had better be dropped, as they are much more apt to be due to distinct traumatism, such as rupture, by the succussion of coughing, of blood-vessels left unsupported by the absorption of the fat

through emaciation, over-distension of the rectal pouches by fæces, etc. Many cases are also probably due to ulceration following the "rupture of an enlarged vein on the surface of a hemorrhoidal tumor while straining at stool," as Ribes first taught.

We cannot refrain from quoting *in extenso* the following :

"You will observe that the term *traumatism* includes injury to tissue resulting from deterioration in quality by malnutrition, injury amounting even to actual death of tissue in minute, or even larger masses, from insignificant or hardly perceptible causes, as well as the more obvious lesions that we usually call wounds. Of all these lesions, which serve as points of origin for abscess, perhaps the most common is the rupture of a small blood-vessel. In this way, by actual lesion of tissue, modern pathology explains the terms 'idiopathic' and 'critical,' formerly applied to abscesses of which the causes were not clear." Again: "I am disposed to emphasize the subject of etiology, because the more thoroughly we grasp the causes of disease, the greater the chances of success by hygiene and preventive measures, and the more direct and rational our treatment." The only point made by Dr. Van Buren requiring special mention is the advice "to paralyze the sphincter by gentle but forcible dilatation" when opening a perineal abscess, "for this measure, by insuring" * * * "greater quiescence for nearly a week, would add, in my judgment, to the chances of a successful result."

Of course, the subject of fistula, which the sixth lecture is devoted to, receives extended attention. It is by far the clearest exposition of the affection that we have yet read. In it an admirable study of its pathology precedes, and logically *indicates*, the treatment. If all surgical pathology was as clearly and scientifically taught, with the logical conclusions drawn as to correct treatment, so that differing methods—too often adopted empirically—were shown to be the real scientific necessary deductions, the foolish prejudice often expressed by some of the elder members of the profession against modern pathology, would be replaced by advocacy of that which they had before despised. The risk of amyloid degeneration from prolonged suppuration, as a reason for operating upon even the slighter forms of fistula, is acknowledged, and Chassaignac's opinion is cited in support of this view. Upon the other hand, there *is* one condition of system where he would withhold the knife, but perhaps not the ligature, viz., "softening and hectic," * * * because "the concussion from coughing, and the lack of power, might prevent the wound from heal-

ing, and the use of the knife, would necessitate confinement to bed, and thus injure the patient." Here is no absurd fear of "suppression of an habitual discharge," etc., but a rational, scientific *pathological* reason. In speaking of the use of the elastic ligature, the great objection to its use recognized is that if branching of the fistula, or several tracts exist, one operation will not suffice. We are, however, at a loss to understand the necessity for "special instruments," as we have operated by this method with ease and success without any such aids. Where a cut high up in the bowel is demanded, the useful suggestion of dividing the tissues by the Paquelin's thermo-caustic knife, instead of the *écraseur*, is made. Where there is a chance that the vital powers are not equal to repairing very extensive incisions, the author advises preparatory slighter operations, and when these wounds are in a satisfactory condition, to complete the procedure by dividing the sphincter. A necessary warning is given against the too prevalent extensive "packing" of the incisions, and it is pointed out that merely enough lint to prevent the healing of the superficial before the deep parts is required. Dr. Van Buren recommends rest in bed for a longer than the usually advised period, as he thinks that the sitting, or upright posture, retards healing—doubtless by favoring venous congestion.

The seventh lecture treats of "fissure, or irritable ulcer of the anus," wherein he gives a needed warning against the misuse of the terms "neuralgia" and "spasm" of the anus, showing how *very* often the troubles complained of are really due to a small *concealed fissure*, which only an ocular inspection will reveal. Forcible dilatation with the thumbs is the method preferred, although the milder modern operation of Syme is accorded its full value. Women with coëxistent uterine or ovarian disease, do not—according to Dr. Van Buren—experience the same benefit from operations for the cure of this affection, and also for hemorrhoids, which others do.

In the next lecture, simple ulcer of the rectum, with syphilitic and chancrous lesions, is considered. In the simplest cases proper diet and applications will usually cure; in more severe cases, incision of the ulcer, with simultaneous division of the sphincter, is advised. In the worst forms, colotomy should be considered not only as a palliative, but as a curative measure.

Benign stricture of the rectum, considered under the three heads of (1) congenital, (2) cicatricial, and (3) fibrous, is ably and exhaustively considered in the ninth and tenth lectures. Several

curious and instructive cases of the first class are detailed. The narrow, "ribbon-like" fæces, which are so commonly spoken of as characteristic of organic stricture, but thought by Curling to be more significant of a contracted or irritable external sphincter, are considered by Dr. Van Buren to be really often due to a stricture higher up, but actually forced "through the opening of the anus," by powerful efforts at defecation, "far enough to give its final impress to solid material extruded under this extreme pressure." He thinks this explanation still more likely to be correct, when we consider that when a narrow stricture has developed, "the external sphincter will be found to have lost, in a great degree, its normal tenacity through non-use," its function having been partially replaced by the stricture. This relaxed condition of the sphincter also explains the involuntary escape of mucus and pus, mingled with fæcal matters, complained of by patients laboring under this affection.

Even in *examining* by the finger a rectal stricture, the utmost caution must be exercised, otherwise the softened gut may be perforated. Attention is called to the singular fact that fistulæ, the result of stricture, usually open *below*, not *above*, the contraction; in this respect, differing from urinary fistulæ, which are, to a certain extent, conservative, opening up new outlets for the obstructed viscus. Occasionally they do act in this way, thus providing a new outlet for the fæces, a case illustrative of it being related.

Dr. Van Buren, although regarding benign stricture as usually—sooner or later—a fatal disease, thinks that life may be prolonged for many years by judicious treatment, and that the new operations now on trial bid fair to result in a possible cure. The unreliability of the bougie as a means of diagnosis, and the preponderating chances of being led into error by it, are well set forth. The author evidently does not believe in "spasmodic" stricture of the rectum, nor do we. Upon the other hand, anæsthesia, "the prone position with elevation of the pelvis," "the use of proper spatulæ and means of illumination, Wales' flexible caoutchouc rectum tubes, so arranged that a bulb of large size can be created by inflating its extremity after complete introduction," * * * and, in "exceptional cases, the introduction of the hand when of small size," Dr. Van Buren thinks, with care and tact, will rarely fail to demonstrate the existence of a stricture. This latter means of diagnosis, he points out, was resorted to more than forty years ago, so that Simon does not really deserve the

credit so often accorded him, other than that of popularizing the method and extending its sphere.

The treatment of benign stricture is considered under the heads of "palliative" and "radical" measures. Diet, laxatives, dilatation by bougies, either with or without partial incision or "nicking" of the stricture, colotomy, and, finally, mercury, iodine, etc., in syphilitic strictures, all belong to the first class. Cures reported after the employment of any of the above-mentioned measures, the author thinks, must be explained by there having been some mistake as to the existence of a stricture at all, or insufficient accuracy in verifying the result, except where the case was one of congenital valvular atresia, or "purely syphilitic stricture treated early." The radical measures are "complete longitudinal division" of *all* the tissues comprised in the stricture; and amputation or excision of the diseased portion of bowel. Of course, where outside pressure has narrowed the gut by compression, appropriate treatment addressed to the removal of any such condition is indicated.

The value of bougies has been over-estimated. They should only be left in the bowel for a short time at each introduction. Aided by incision, these instruments are perhaps curative in "thin-edged congenital valvular strictures." Judiciously and assiduously used, they often prolong life indefinitely. Division and rapid dilatation have proved both useless and fatal. Whitehead's elastic dilator is merely mentioned. Having tried it, we think that it deserves more than this, especially in conjunction with nicking. Dr. Van Buren does not speak highly of this latter method. The author points out the important fact that antisymphilitic remedies have no more effect on a stricture due to syphilis, *when it has reached the stage of fibroid induration*, than they can on an ossified periosteal node, so that the argument against the existence of a variety of stricture due to this disease, drawn from the failure of specific remedies, is fallacious. Colotomy in appropriate cases is extolled as a valuable palliative and perhaps curative agent. The question whether lumbar or inguinal colotomy is the better operation, the author considers undecided; but he is inclined from his experience to favor the latter operation. We regret that the various operations are not described, an omission which is rather a grave fault in a work such as this.

As a radical measure, complete longitudinal section of all the involved tissues is recommended as an alternative for colotomy. Dr. Van Buren thinks that this plan offers a "fair promise" of

cure in certain cases. The incision should be made, the author thinks, to one side, as the external sphincter when divided in the median line seems reluctant to heal. The thermo-cautery knife is advised, as it insures against hemorrhage and protects the raw surfaces by the eschar formed from contact with the fæces. Amputation or excision in certain cases where there is a suspicion of malignant disease is suggested instead of the "complete longitudinal section," and as preferable to colotomy.

The eleventh lecture treats of cancer of the rectum in an able and instructive manner, but as it contains nothing strikingly new, the reader is referred to the original.

Dr. Van Buren makes one pregnant remark, however, viz., that the microscope of late has demonstrated the slender differences existing between benign and cancerous disease of the rectum, thus increasing the probability of the local origin of malignant disease in this locality, "and consequently the greater hopefulness of operations undertaken for its early removal."

Congenital malformations, fœcal impaction, foreign bodies, atony of rectum, neuralgia of the anus, hygiene of the lower bowel, with diagnosis and exploration, are considered in the twelfth and concluding lecture. Admirable throughout, we would emphasize certain points. In operating for imperforate rectum, he warns against the *ultimate* uselessness of dissecting through the perineum, unless, as Amussat first succeeded in doing, the upper rectal cul-de-sac can be dragged down and secured to the edges of the perineal incision. Dissection, to reach this pouch, should never be carried beyond one inch and a half, aided, perhaps, by excision of the coccyx; and "thrusting a trocar beyond the reach of the finger in search of meconium," is declared to be, "under any circumstances of imperforation," * * * "an unjustifiable proceeding." Should the perineal dissection fail, Littre's operation should be done. The uselessness of purgatives, as a rule, in fœcal impaction is rightly insisted upon, and mechanical removal inculcated. The ultimate danger to life from foreign bodies in the rectum, from peritonitis, are pointed out, with the minor evils of abscess, fistula, rectitis, local gangrene, etc. If ordinary measures fail after forcible stretching of the sphincters, the hand, if small, should be introduced. As a final resort, laparotomy should be considered. It has been already resorted to with success in four cases. Several useful instruments are recommended as aids to diagnosis, such as a modified Sim's speculum, in conjunction with a somewhat similarly-shaped boxwood instrument and Wales,

flexible rectal tubes. We quote the following, as it is worthy of the most careful consideration, as the outcome of the experience of one long accustomed to teaching: "Accurate diagnosis is, in our profession, the unerring test of ripe scholarship and thorough education; and of all the qualities of a physician it is that which most certainly insures success in curing disease, and consequently reputation. Imperfect diagnosis * * * is a very common fault, especially so * * * in the class of ailments which we are studying, for the seat of them * * * is hidden away, as it were, in a recess of the body, and natural modesty is always averse to exposure. Moreover, our means of exploration have been, until recently, very defective,—entirely insufficient to overcome satisfactorily the jealous sentinelship of the sphincter ani muscle." Under the hygiene of the lower bowel, Dr. Van Buren denies that any one is "naturally costive," and points out that there is some sin of omission or commission which accounts for the condition. This must be sought for and removed. Excellent advice as to palliatives is also given.

The author concludes by saying that he has endeavored to make these lectures "suggestive rather than exhaustive." We think that he has done far more, and congratulate him upon having produced probably the best work written on diseases of the rectum. Dealing somewhat too little in the details of certain operations, it is yet so admirable that every one who can should possess a copy, or, at least, carefully read the book. It is well printed, the woodcuts are fully up to the average, and, altogether, the publishers have done themselves more credit than by the last medical book that we examined issued by the same firm. Dr. Van Buren is evidently a careful proof-reader, for we have rarely, if ever, examined a book with so few typographical errors. [C. B. N.]

A Manual of Diseases of the Eye and Ear for the Use of Students and Practitioners. By W. F. MITTENDORF, M.D., Surgeon to the New York Eye and Ear Infirmary, etc. 445 pp. New York: G. P. Putnam's Sons, 1881.

With the exception of a small treatise, published some time ago, by Dr. H. W. Williams, we believe this is the first text-book on diseases of the eye which has been offered to the profession by an American ophthalmologist.

When one takes up a new book which proposes to go over already well-trodden ground, there is a natural curiosity to discover its *raison d'être*. As we already possess several excellent text-books and treatises on ophthalmology in the English lan-

guage, we were not a little curious to learn why we should have another. We naturally turned to the preface of Dr. Mittendorf's book to discover the usual "want long felt." And we found it as follows: "The want of a short, practical manual of the diseases of the eye and ear in the English language has long been felt by the medical student; I have, therefore, at the request of many members of my private classes, given in this little book my lectures upon these subjects, somewhat enlarged."

The propriety of multiplying text-books, and particularly manuals upon such distinct branches of medicine as ophthalmology and otology, which are designed "for the use of students and general practitioners," may well be questioned. A student who intends to give himself to the special study of these two branches will generally require something much more extended and thorough, and the general practitioner who proposes to treat eye and ear diseases at all will also find it to his advantage to consult authorities which enter much more into the details of symptoms, diagnosis, and treatment. Still, there is, unquestionably, a great desire among students and fresh graduates to have all the principles of the various divisions of medical study, particularly the "specialties," given to them in a nut-shell. It is a labor-saving dodge, which, as they afterward discover, is not so successful as they had hoped for; but each succeeding generation feels, for all that, the same "want," and it must be supplied. As Dr. Mittendorf's book has no higher object than the supplying of this want, it would be unfair to judge it by any other standard.

It possesses, too, one advantage which no other book of the kind does,—it contains the elements of two specialties boiled down. This is particularly a recommendation to the student and practitioner in this country, where the practice of ophthalmology and of otology are, as a rule, united.

As we come to examine the work, part by part, we find the subjects treated in a clear and concise manner; and considering the temptation, which must be strong under these circumstances, to elaborate, the author has held himself well in check, and, as a rule, has left untold those things which could best remain untold, and has given prominence only to what are most generally considered to be the important facts. He has quite thoroughly posted himself on the literature of his subjects, and even the most recent additions to our knowledge are given, if only in a single line. There are evidences of rather careless proof-reading, which, we are sure, will not be apparent when we come to notice the second edition.

The colored plates at the end of the volume (copied from Liebreich's and Politzer's atlases) will, no doubt, appeal to the student who has not access to the originals nor to the excellent atlas of Jäger. For ourselves, we would be glad if the copies were better, but they will, no doubt, serve quite as good a purpose as the more highly-finished originals. For, after all, even the best drawings are only signs of things, and can at best give us but suggestions of the conditions they are intended to represent. [S. M. B.]

Lectures on Diseases of the Nervous System, especially in Women. By S. WEIR MITCHELL, M.D., etc. H. C. Lea's Son & Co., Phila., 1881, pp. 233.

Dr. Mitchell's books are always looked forward to with interest, and command a large and sympathetic audience; and this is justly the case, for although in reading them we find ourselves often wishing, just because they are so excellent, that they might be still better, yet they possess the prime requisites of good medical books, in that they are conscientious records of experience, and that they bear the stamp of a clear, shrewd mind, and senses naturally accurate and acute.

Many of his practical suggestions and investigations are marked by a boldness and originality born of real genius. To this it is due that his most important work, the "Injuries of Nerves," is still largely quoted, the world over, notwithstanding the fact that it was lacking in the completeness and the encyclopedic qualities of a good monograph, and that the arrangement of its parts was unsystematic and confusing.

Without this explicit recognition of the high merit and great practical value of all Dr. Mitchell's work, and of the fact that most of it was done under pressure, and, practically, would never have appeared if it had been retained till properly perfected, it would not be fitting to proceed to a closer examination of its faults. These are, however, equally characteristic, and consist essentially in the fact that his subjects are rarely treated with systematic thoroughness, or with due attention to scientific accuracy of analysis in matters of detail, when the latter are not special objects of inquiry at the moment.

Dr. Mitchell is, in fact, by nature a pioneer, with the virtues and the strength, but also with some of the faults of the class. Thus, he does not seem to us sufficiently sedulous in searching to see how far the facts which he observes so accurately and describes so well have been already observed and described before. This very book will illustrate the criticism.

His style is always engaging, often very picturesque and telling, and a book of his, once taken up, is not willingly laid down till it has been read through to the end.

This gift of graphic writing is, however, not one for which he is to be wholly congratulated, so long as his themes are of a kind which excite popular interest, like those which he has dealt with of late. For it attracts a class of lay readers who would much better leave such literature alone ; and it leads the author, we venture to think, to prefer the style of the lecture to that of the monograph, at the cost of sacrificing a certain amount of exactness or thoroughness of thought with regard to points which, though of scientific importance, lie somewhat outside of the line of argument which he may happen to be following.

The same merits and the same faults reappear in almost every one of his long list of shorter papers. They are always marked by originality and proofs of conscientious accuracy of observation, but are often more or less deficient in the signs of exhaustive examination of the opinions of others, which may make a book dull reading to be sure, but which the reference-loving student of the present day justly looks for.

A more liberal infusion of the best German spirit, which seeks to throw a veritable calcium light of criticism upon every detail of a scientific work, would greatly enhance the value of these, though we fully admit that rather than have them abandoned from the impossibility of giving them this treatment, we would much prefer to take them as they are.

The volume now before us is not a text-book, nor a complete set of lectures, but a series of essays in lecture-form, each embodying some argument or illustration of great interest.

In brief, they may be said to deal with new or striking illustrations of old phases of hysteria, with investigations into chorea, and with a reiteration of the mode of treatment, which his "Fat and Blood" has made so famous.

The histories of hysterical cases which he cites will be found full of interest for those dealing practically with like cases, though they offer few facts which are actually new. Thus, the characteristics of his motor ataxia, *i. e.*, without disorders of sensation, cutaneous or muscular, are distinctly expressed by Jaccoud (Paraplégie, etc., 1864, p. 631), if not by others.

The hysterical auras of sight, hearing, etc., also are described, not, to be sure, so well, by Jolly in Ziemssen's Cyclopedia ; yet neither of these two writers are referred to, though they are standard authorities.

The criticism comes back again to that already expressed, viz., that the lecture-style is one not adapted for serious work, such as might fairly be expected of an author of Mitchell's ability. It is too easy, and too little binding on the writer, and withal does not permit of thorough treatment of the subject.

Some of the statements about the pulse-rate are very interesting, especially those with regard to the loss of the usual harmony between it and the rate of respiration.

Dr. Mitchell has seen two cases of sudden death in hysteria, said to have been due to "congestion of the kidneys."

We suppose this means some form of nephritis—though we wish the expression had been more explicit,—and if so the observation is of interest as harmonizing with a recent statement by Dujardin-Beaumetz, that he also has seen two cases of sudden death in hysteria from renal disease, one of them an interstitial nephritis, the other a "complete degeneration" of the kidneys.

These facts give us one warning the more not to overlook signs of real organic lesion in the midst of a profusion of functional symptoms.

Mitchell refers, in another place, to this danger, and speaks, with Charcot, of the possibility that hysteria may terminate in sclerosis. This important matter, as to the relation of hysteria to organic disease, has been treated of at some length by Dr. E. C. Seguin, and referred to by various other observers.

We question, with Jolly, in Ziemssen's *Cyclopedia*, whether it would not be safe to speak of sclerosis as sometimes coëxisting with hysteria, or following it, rather than as its outcome.

If the latter view were really correct, some light, though a vague one, would be thrown upon the nature of the nutritive conditions present in the nervous centres during the hysteric state. Yet if there is this organic connection between these disorders, why is not the sequence referred to more common?

The differential diagnosis between hysteria and disseminated sclerosis is certainly at times difficult to establish with certainty, as Jolly admits, and if, as appears, the tendon reflexes may be exaggerated in pure hysteria, it might well at times be impossible. A similar difficulty is shown by Mitchell to exist as to the diagnosis between real and hysterical or mixed forms of athetosis.

There is a wide difference, however, between similarity as to functional manifestation, and similarity as to organic cause.

In the chapter on hysterical motor ataxia, the author states in passing that he has actually measured and proved the retardation of nerve-conduction in some hystero-palsies.

These observations have failed to meet our eye, but we should certainly regard them as of great value. It has always seemed as if it ought to be possible to learn something about the functional efficiency and nutrition of the nerve tissues, in these neuroses, by more exact methods than through bed-side observation, yet first-rate investigations of this nature are few. Exner was not, we believe, able to discover that the rapidity of his cerebral reflexes varied with the temperament of the person experimented upon. In chorea and in tetany, to be sure, changes in the irritability of the nerve trunks has been observed, but we know of no time-measurements.

It is in the analyses, which occur throughout the book, of the different varieties of the hysterical temperament, that Dr. Mitchell's gifts, both of observation and expression, show themselves, perhaps, to their best advantage, and it is plain that he has succeeded in utilizing to an unusual degree, for the good of his patients, his insight into their characters.

It may well be that he is a little Procrustean in fitting his types and his individual cases together, still the interpretation which he gives his patients of their own mental states, is no doubt recognized by them as, if not exact, often a better one than they themselves could furnish.

Two of the chapters in the book are taken up with an interesting study of chorea, in its relation to climate, locality, race, and other conditions.

After reaffirming his former statements as to the great frequency with which chorea occurs in the early spring time, he explains some tables, prepared by Dr. Morris J. Lewis, showing curves indicating the time of onset of a considerable number of cases of chorea in the different months, during several years, in Philadelphia, and other curves indicating the temperature, the rain, and snow-fall, the relative humidity, the height of the barometer, etc., and, finally, the passage of storm-centres within 750 and 400 miles of Philadelphia during the same period.

Between the curves of chorea and those of the storms (and them almost alone) a most striking and unmistakable similarity exists.

This interesting observation recalls the analogous one made and published some years ago with the aid of a patient, Capt. Catlin, a sufferer from stump-neuralgia, as to the relation between pain and weather. The exact element in the storm-influence which produces either the neuralgia or the chorea still eludes detection.

Equally important is the discovery that poliomyelitis infantum is in Philadelphia far more common during the hot months, indeed increases in frequency in proportion to the heat.

We do not know to what extent the Philadelphia observations with regard to chorea have been confirmed in other cities, but in Boston a similar, though not the same law as to season of onset holds good. An unpublished analysis of 146 cases, by Dr. H. C. Haven, shows that in Boston the largest number of cases occurs in the spring, the next largest in winter, the smallest in the fall ; or, 101 cases in the first six months against 45 in the last six.

The suddenness of the increase between December and January is very striking.

Passing over several chapters on hysterical disorders of sleep, vaso-motor and gastro-intestinal disturbances in the hysterical, etc., which contain quite a rich collection of clinical observations, we may dwell for a moment on the last chapter, devoted to a re-statement of his famous treatment by rest, seclusion, feeding, etc., now so widely and unfortunately so popularly known.

Dr. Mitchell's position with regard to this treatment and his confidence in its value is unchanged, though he speaks with greater moderation about the possibility of treating these cases of nervous break-down at home, if the physician is willing to impose the necessary strain upon himself.

He evidently contemplates, and justly, with much pride, the fact that the verdict of many good observers at large has been distinctly to the effect that this method, developed as it has been, is a valuable addition to our therapeutics.

Not that it may not have been applied before, from time to time, in its essential features, to the treatment of these same bug-bear cases, but that no authoritative teacher, in our time at least, had offered a system so complete, so well-reasoned out, and withal so simple. We believe, however, that its greatest—and that a very great—merit, lay less in the suggestions on food, massage, etc., than in the fact that it furnished a safe and efficient method of obtaining from the doctor and the patient herself that moral control over symptoms of nervous excitability, which is the prime condition of the cure, and which coaxing, and commanding, and the rest of the mental tonics ordinarily prescribed usually fail to secure.

Looked at from the strictly scientific stand-point, Mitchell's system is somewhat rough and ready, and still needs, no doubt, revision and amplification.

It would be well worth investigation, for example, whether it might not be modified to advantage, by the addition of some of the hydropathic measures which are believed to affect the nutrition so profoundly, such as the cold pack followed by massage, as recently advocated.*

One of the results claimed for this is that it flushes the muscles with blood, and thus places them in a more favorable condition for deriving benefit from the subsequent manipulation. Its action in anæmia is said to be more favorable than in neurasthenia. These and similar points are deserving of further study, which they will doubtless receive from Mitchell or some one of his colleagues.

Before finally taking leave of this matter, we cannot but express regret, in spite of the explanations given in the second edition of "Fat and Blood," that more is not attempted in the way of statistical analysis of the cases treated upon the plan of which so much has been said. It is now many years that Dr. Mitchell's well-merited success has been attracting large numbers of patients to his care. His cures have been many, but among them and besides them, one hears, both from Dr. Mitchell himself and from others, of occasional failures and many relapses. It would be of the highest interest to know under what influences and conditions these results happened, and with what frequency, in order that future, and perhaps younger workers in the same field may not be led to reproach themselves unnecessarily when they see their first pet cases turn out badly in the end. [J. J. P.]

* On the use of the cold pack, followed by massage, in the treatment of anæmia, by Mary Putnam Jacobi, M.D. and Victoria A. White, in ARCHIVES OF MEDICINE, vol. iii, p. 296, vol. iv, p. 51.



